Seasonality, Academic Calendar and School Drop-outs in Developing Countries*

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Abstract

Rural families face tradeoffs when deciding whether to keep children in school or have them work in the field. School calendars can magnify this tradeoff by not accommodating agricultural harvesting cycles within the schedule. We show this misalignment has a significant and sizable effect on school continuation. In Bangladesh, a rise in seasonal labor demand due to the *Aman* paddy harvesting typically coincides with the yearly final examination of schools. Employing the lunar calendar variation of *Ramadan* school holidays as a natural experiment framework — that forced schools to re-schedule final examinations to a pre-harvest season in 1999 — and comparing it with a typical year of 2002, we find that annual exams overlapping with major local harvesting period inflate the school dropout by 6.5 to 8.4 percentage points between the agricultural and non-agricultural households. Age-specific cohort analysis using a nationally representative household survey also supports this evidence. Exploiting state-level academic calendar variation, we executed a similar analysis for India and found supporting evidence to validate our findings. Our paper suggests the careful design of school calendars in developing countries by adequately addressing local seasonality.

Keywords: Enrollment; child labor; seasonal labor-demand; school calendar; ramadan; drop-out.

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1 Introduction

Improving access to education has been a top policy priority for many developing countries (Birdsall et al., 2005). In addition to supply-side policies in the form of greater infrastructure support, developing countries are increasingly focusing on demand-side interventions like free schooling and conditional cash transfer programs.[1] These programs are effective in reducing the opportunity costs of schooling born by the households — helping the demand for education to rise (Saavedra and Garcia, 2013).[2] Despite these policy supports, lower school continuation and higher levels of dropout have been major challenges faced by the developing countries. Scholars and practitioners have identified many socioeconomic issues, such as poverty, credit market imperfections, social norms, and traditional gender roles as leading causes of students discontinuing schooling. One important (but ignored) issue concerning school continuation is the seasonal labor demand conflicting with the schooling calendar, which received limited attention in the literature.

Local seasonality, which is an important dimension of rural livelihoods, can seriously affect school continuation of many poor students in developing countries. Low-income families, who are mostly credit constrained and often dependent on their children's labor, face a

[1] Research on the demand-side aspects of schooling suggests that the household's opportunity costs of schooling can be exceedingly high for poor households. This is due to liquidity constraints (Jacoby and Skoufias, 1997; Beegle et al., 2006), inability to insure against shocks on income-earning capacities (Jensen, 2000; de Janvry et al., 2006; Case and Ardington, 2006), comparative advantages in physical remunerative works (Pitt et al., 2010), or children's inability to become decision-makers for their own human capital investments (Baland and Robinson, 2000).

[2] Despite progress in attendance and enrollment rates, examination scores reveal that demand-side policy interventions have not led to significant changes in learning. This should not necessarily be regarded as a lack of success on the part of these programs. The fact that learning has not deteriorated despite higher attendance can be considered a partial success. Similarly, learning is often associated with supply-side issues: if the quality of schooling is low, increasing its demand will not result in improvements. Education quality is an outcome of classroom conditions, infrastructure (Lockheed et al., 1991), teacher presence (Banerjee and Duflo, 2006), teacher ability, teacher efforts (Duflo et al., 2009; Atherton and Kingdon, 2010), teaching materials (Glewwe et al., 2004), and school curriculum (Duflo et al., 2009). (Among others, see also Lockheed et al., 1991; Hanushek, 1995; Glewwe and Kremer, 2006, for reviews). Recent studies show that teachers respond to attendance incentives (Duflo et al., 2012) and examination score incentives (Muralidharan and Sundararaman, 2011), which have resulted in improvements in India. However, it is important to note that several rigorous evaluations using randomized control trials (RCTs) have failed to find any impact on school quality from various cost-effectiveness measures. For example, children's test scores did not improve from spending on resources such as textbooks (Glewwe et al., 2009), flip charts(Glewwe et al., 2004) or additional teachers (Banerjee et al., 2007).

fundamental tradeoff when deciding whether to keep children in school or have them work in the field.[3] In agrarian societies, this tension is greatest during the harvesting season that experiences the rise in labor demand, which in turn increases the wage rates and pulls students out of school. This pressure of seasonal work is not typically addressed in education-related policies in developing countries. In this paper, using a natural experiment framework, we estimate the adverse impact of this seasonal labor demand that conflicts with the annual school examination calendar in Bangladesh. We find this overlap is causing 6.5 to 8.4 percentage points higher school dropout for students from agricultural households compared with non-agricultural households. To check the external validity of this finding, we conducted a similar analysis for India, where we estimated this calendar conflict leads to 5.3 to 6.6 percentage point reduction in the enrollment. These are important findings, which are both statistically significant and economically sizable, caused by the inadequacy in addressing local agricultural seasonality in education policy — a lesson for many developing countries trying to achieve universal education and lower school dropout.

In Bangladesh, the schooling system follows the English (Gregorian) calendar for academic activities (January-December), which does not accommodate the local agricultural cycles. This misalignment of the schooling calendar follows a legacy introduced during the British Colonial period's education reform — which was primarily targeted to the elite and affluent classes of society — to provide clerical and administrative supports to the colonial administration (Rahman et al., 2010). As a consequence of this misalignment, annual examinations in schools, which are typically held at the end of the calendar year (specifically

[3] Edmonds (2007) indicates that the concurrence of schooling and work is a universal phenomenon among UNICEF's middle-income countries (MICS). Furthermore, Ravallion and Wodon (2000) demonstrate that child labor and schooling are not mutually exclusive activities for school-age children, despite their competing use of time. Studies examining the overlap in children's time allocations consider the possible consequences on the human capital acquisition. These results, although mixed, are mostly negative. Akabayashi and Psacharopoulos (1999) show negative correlations between exam scores and work hours in Tanzania. Using a data set from Ghana, Heady (2003) finds negative correlations with outside work hours but not with housework hours, even though outside work hours are relatively short. Gunnarsson et al. (2006) examines country-level data-set of nine Latin American countries and finds a negative association between work and exam scores. Furthermore, Dumas (2012) also confirms the concurrence of work and schooling, but shows that there is no negative impact on future exam scores by using the panel data from Senegal.

in December), coincide with the peak harvest period of the major wet season paddy called *Aman*.[4] *Aman* rice harvesting period is between late November and late December, with the seasonal labor demand for the harvest peaking in December. During the harvesting period, children's schooling gets routinely interrupted through active involvement with production and post-production processes. Unlike Bangladesh, where a national homogenous school calendar is utilized, in India, this conflict occurs due to the variation in the state-level school calendar and local major crop harvesting season, making some states' academic routine unfavorable for the students from agricultural households.

Poor agricultural households, who typically cultivate small-hold tenure land, are generally unable to hire external labor because of resource scarcity, increased wages, and credit constraints.[5] Such households involve their children and other household members in assisting with harvesting and associated post-harvesting tasks (threshing, husking, storage, transportation, and selling end products to the market).[6] Moreover, the opportunity cost of schooling increases during the harvesting time, as the market wages of child labor rise.

Consequently, school-going children of agricultural households regularly remain absent from school for an extended period. In a detailed education assessment study by USAID, 42% of rural students were reported to be absent during the harvesting period in Bangladesh (Rahman et al. (2004), Table IV.D.9, page 110). This is not just a past phenomenon; a recent 2017-18 study on school students in Bangladesh also found a similar trend of absenteeism for rural children (Fujii et al., 2019). This is partly due to the homogeneous academic calendar in Bangladesh, with a strict time-table for all the schools to hold the year-end final examination in the first two weeks of December — a high demand season for seasonal labor (see Figure 4

[4] *Aman* rice is the largest crop in Bangladesh by area and the second-largest by cereal production.

[5] Even if they can manage to do so, parents who are myopic or not entirely interested in investing in their children's human capital as a result of their conflicts of interest, may choose not to (Baland and Robinson 2000).

[6] In addition, many landless families depend on seasonal agricultural work opportunities. The adults of these families — predominantly male — work extensively during the harvesting period, which requires frequent migration out of the village, while children in the household take care of the livestock and other activities (like fetching water and hey-stacking for fodder).

in the appendix).[7] Similarly, high absenteeism is reported in India's rural schools due to such conflict with the agricultural calendar as reported in the paper by De and Mehra (2016).

Children involved in harvest labor also face a greater risk of injury due to the use of traditional tools such as sickles. This, along with work-related fatigue, caused by the physically demanding harvesting work, and inadequate night-time lighting at home — as these students need to study in the evening — also hampers their exam preparation. All these factors result in children achieving lower academic scores or missing the exams — leading them to discontinue schooling. The progression to the next grade is usually contingent on satisfactory performance in the annual grade completion examination held at the end of each academic year (BANBEIS, 2007).[8] Technically, a student can repeat the grade due to unsatisfactory performance in the yearly final exam. However, grade repetition is not encouraged and practiced; as a result, failing a grade typically leads to school discontinuation.[9] It is easy to assume that children who drop-out are poor students and are making a rational choice. Nevertheless, the conflicting academic calendar and local agricultural cycle adds an extra burden to the problem.

Education practitioners have acknowledged the importance of seasonal labor impacts on dropout rates (Ardt et al., 2005; CAMPE, 2004, 2008; DPE, 2009; Hadley, 2010; De and Mehra, 2016) and urged for a flexible academic calendar to cater for such seasonality

[7]Even the primary school terminal exam, known as Primary School Certification Exam (PCSE), which was introduced in 2009, was scheduled in the last week of November in 2010. Unsurprisingly the Directorate of Primary Education reported a 10% absenteeism on the first day of this largest nationwide public examination, which was highlighted in the major media outlets, including in the leading English newspaper *the Daily Star* (click here `https://www.thedailystar.net/news-detail-163453`). Interestingly on the same day, the 24th November 2010, the Daily Star reported bumper production of *Aman* paddy in some parts of Bangladesh (click here `https://www.thedailystar.net/news-detail-163434`).

[8]It is true that exams are not held every day of the two months of harvesting period, but the issue we are focusing in this study is more on the systematic unpreparedness for the final exams during the harvesting period. Other than that, schooling and working trade-off, missing classes, fatigue, injury, and lack of academic supports at home to catch-up (especially for the first generation learners) also cause students from agricultural households to under-perform in the final exams, which lead to failing the final exam and eventually dropout.

[9]According to the data available from the World Bank, the percentage of repeaters in Primary education is only 4% and 1% for Bangladesh and India, respectively. `https://data.worldbank.org/indicator/SE.PRM.REPT.ZS`

(Rahman et al., 2004).[10] However, except by Sabates et al. (2010), this issue has not been examined in the academic literature. Previous studies have examined the role of technology and price changes in agriculture on schooling, but have not examined the changes in labor demand related to agricultural seasonality.[11] This is partially due to the difficulty of verifying such claims, as local seasonal labor demand and local productivity shocks are not easily observed. As a result, current literature does not inform us how much of the annual enrollment rate variation can be explained by the calendar misalignment with agricultural seasonality.

To address this knowledge gap, in this paper, we estimate the impacts of increased seasonal labor demand at the time of the annual final exam on school continuation. An ideal way to conduct such an evaluation is to set-up an experiment by employing a Randomized Control Trial (RCT). However, implementing an RCT in this context, which would involve assigning different examination schedules to different individuals, is expensive and logistically demanding, as one must randomize at the level of schools, which necessitates a large-scale operation. Instead, this paper primarily utilizes the mandatory school holidays given during *Ramadan* as a natural experiment that forced schools to bring forward their final examinations to the pre-harvest season in Bangladesh, a time of reduced local agricultural labor demand.

Bangladesh is predominantly a Muslim country, and fasting during *Ramadan* is a compulsory activity among the Muslims. During *Ramadan*, schools are instructed to provide holidays to accommodate fasting activities for children. However, the schedule of these holidays is not fixed as Islamic months follow a lunar calendar system. As a result, *Ramadan* drifts by 11-12 days per year based on the solar calendar. Interestingly, in 1999, *Ramadan* was

[10] A recent comprehensive study investigating the reasons for rising dropout rates in Bangladesh (DPE, 2009) reports that child labor is the second most frequently cited cause for dropping out after poverty. Because poverty and child labor are mutually related, an academic calendar that does not accommodate the seasonal labor demand may also have contributed to the rising dropout rate.

[11] Some studies find negative estimates of child schooling on their wage and yield variations (Rosenzweig and Evenson, 1977; Jacoby and Skoufias, 1997), while other studies found that a persistent change in productivity increases enrollment (Rosenzweig, 1990; Foster and Rosenzweig, 1996, 2004).

celebrated in December; consequently, schools had to move their annual final examinations one month backward, to November (the off-harvest season for *Aman* rice) to accommodate the completion of the academic schedule as well as the holidays. This exogenous shift in the annual examinations creates a candidate for a natural experiment framework that allows us to estimate the impacts of the school calendar on academic continuation. Using household-level panel data of 1999 and 2002 and employing first-difference (FD) estimator in the difference-in-differences (DID) setting, we compare the enrollment rate changes for agricultural and non-agricultural households to assess the differential impacts of seasonal labor demand changes on school continuation. To check the common trend assumption necessary for the consistency of the DID-FD estimator, we show that this is likely to hold by using data set on later rounds (2002 and 2006). In addition, we controlled for time-variant variables such as local paddy yield and weather. We find that annual exams overlapping with the *Aman* harvesting period increases dropout for children from agricultural households by 6.5 to 8.4 percentage points (from the base of 45 to 50 percent enrollment by the non-agricultural household children). We also compared our estimates with Muslim and non-Muslim households to disentangle any confounding factors such as festivity and fasting. We found these to have negligible impacts. Our finding survives a battery of robustness checks, and placebo tests, and maintains low p-values as well as economic sizes. Moreover, we executed a similar analysis for India as an external validity exercise by exploiting the state-level academic calendar variations. As with our findings in Bangladesh, we found a similar impact of academic calendar mismatch on school continuation for India's agricultural household children.

We checked the overall impact of *Ramadan* induced change in the academic calendar on educational outcomes in Bangladesh. Using the latest round of the nationally-representative Household Income and Expenditure Survey, HIES (2016), we conducted age-specific cohort analysis on academic outcomes in rural areas. Our analysis suggests that the school-going age cohort in 1999 significantly benefited from this favorable academic calendar, which raises 0.46

years of education and increases the probability of completing primary, secondary and higher secondary schools by 15, 16 and 14 percentage points, respectively. This impact generates about 3 percent average economic return for the benefited cohort due to a favorable academic calendar.

This finding of the academic calendar conflicting with seasonal agricultural work and leading to school dropout has broader implications and is not limited to Bangladesh or India. In Africa, temporary withdrawals of children out of school during the harvesting and times of hungry-season-led migration results in permanent withdrawal from schools (Hadley, 2010; Andvig et al., 1999). In the case of Ethiopia, school enrollment begins in September; however, children leave schools in November due to the harvesting labor demand of *meher* season crops,[12] leading to longer school absenteeism and eventual dropping-out (Colclough et al., 2000). Some researchers have argued that this conflict of seasonal work and schooling is the most important reason for school dropout in Ethiopia (WorldBank, 1998). In Kenya, where a large portion of casual agricultural workers are underage children, a similar problem occurs as maize harvesting (October-November),[13] overlaps with the Kenyan Certificate of Secondary Exam (KCSE) in November.[14] A similar conflict is observed in other counties such as Malawi (Kadzamira and Rose, 2003), Nigeria, and Nepal.[15] This issue becomes even more complicated due to multiple climatic zones coupled with country-wide uniform school calendar. Countries such as Tanzania, Brazil, Colombia, and India have multiple climatic zones suitable for different crops, which creates academic calendar conflicts with local agriculture seasonality. In Bangladesh, the southern coastal zone faces different seasonal labor demands compared to the rest of the country due to shrimp harvesting and fishing (DPE, 2009). Similarly, higher incidents of seasonal child labor have been observed among

[12]These crops are Barley, Maize, Wheat, Sorghum, Oats, and Millet.

[13]see `http://www.fao.org/giews/countrybrief/country.jsp?code=KEN`

[14]see `https://en.wikipedia.org/wiki/Kenya_Certificate_of_Secondary_Education`

[15]In Nigeria, maize, which is the principal crop, is harvested between late June and early August. This harvesting season overlaps with the Senior Secondary School Exam (SSCE), typically held during June- July. Similarly, in Nepal, district level examinations such as the Basic Education Examination (BEE) and national Secondary Education Exam (SEE) are typically held in April, overlapping with wheat harvesting season. This information have been compiled from Food and Agriculture Organization (FAO) and Wikipedia.

coffee pickers in Kenya (Birger and Craissati, 2009), on rubber plantations in Tanzania (Andvig et al., 1999), and in cotton production in Turkey (Gulcubuk, 2010) — all conflicting with the academic calendar and leading to school discontinuation.

Our finding suggests that policymakers in agrarian economies may need to revise the school calendar and curriculum to accommodate local labor demand seasonality to minimize its potential adverse effects on school attendance, learning, and continuation. Such a revision would complement demand-side policies in addressing the tradeoffs faced by students explicitly. Education reforms in developing countries often focus on teacher incentives, ICT adoption, and better curriculum design; the academic calendar's adjustment has not received due attention. This issue is becoming increasingly important as developing countries aim for greater outreach of universal education and education-related Sustainable Development Goals (SDGs), which require precise targeting for rural school-going children.

In the following section, we first present a simple theoretical framework. In Section 3, we discuss the identification strategy of the paper and other strategies to separate potential confounding factors in the estimation. In Section 4, we use descriptive statistics to examine the data and explain our method for selecting the sample. Section 5 presents the estimated results, section 6 discuss the external validity of the finding, and Section 7 concludes. Detailed discussion on the data and robustness checks are presented in the appendix.

2 Theoretical Framework

In this section we use Baland and Robinson (2000) to show a simple theoretical framework to better our understanding of the impact of seasonal labor demand coinciding with the examination period. Consider an individual living over two periods. In the first period, she faces a tradeoff between the optimal schooling hours l, and work $1-l$. If she chooses school for l hours, she receives an income according to the production function $h(1-l)$, and her second period income y increases at rate $e(l) > 0$ with $e(0) = 1$. We let a multiplicative term

$1 + aD$ where $a > 0$ measure the productivity change in production. In harvest seasons, D takes the value of 1, and 0 otherwise. Rewriting $1 + aD = m$, the individual's problem is as follows:

$$\begin{aligned} \underset{c_1,c_2,l}{\text{maximize}} \quad & u(c_1) + \beta u(c_2) \\ \text{subject to} \quad & mh(1-l) = c_1 + s, and \\ & e(l)y + Rs = c_2, \end{aligned} \tag{B1}$$

where we denoted c_t as period t consumption with $t = 1, 2$, $\beta \in (0, 1]$ as a discount factor, s as savings, y as second-period base income, and $R > 0$ as an interest rate factor. Upon substitution, this is equivalent to the following:

$$\max_{\{s,l\}} u[mh(1-l) - s] + \beta u[e(l)y + Rs].$$

First-order conditions (FOCs) are as follows, assuming positive savings [16]

$$-u'(c_1) + \beta R u'(c_2) = 0, and$$
$$-mh'(1-l)u'(c_1) + \beta e'(l) y u'(c_2) = 0.$$

The second FOC suggests that individuals equate marginal utility loss of income due to schooling in the first period to marginal utility gain due to increased income in the second period. Substituting the first FOC to the second FOC, we have the following:

$$e'(l) = \tfrac{R}{y} mh'(1-l). \tag{B2}$$

[16] Alternatively, one can assume that the interest rate is an effective interest rate that varies according to household wealth and other entitlements.

If there is a uniform market wage rate w, then at the equilibrium without any factor market imperfection, we must have $w = mh'(1-l)$. Then the above becomes

$$e'(l) = \tfrac{R}{y} w. \tag{B3}$$

Let us assume that the return to schooling e and production h are strictly concave functions. In addition, assume that regularity conditions $\lim_{l \to 0} e'(l) = \infty$ and $\lim_{l \to 0} h'(1-l) = \bar{h} > 0$ hold.[17] There exists $l^* > 0$ that satisfy FOCs, because the left-hand side of (B2) is increasing while the right-hand side is decreasing in l. When $D = 1$ (and $m > 1$), the marginal productivity of labor increases and l^* decreases.

We can alternatively rewrite (B2) as

$$\begin{aligned} g(l) &= \frac{R}{y} m, \\ g(l) &:= \frac{e'(l)}{h'(1-l)}, where\ g' < 0. \end{aligned} \tag{B4}$$

Taking an inverse function of $\frac{e'}{h'}$, we see that $g(\cdot)$ is nonlinear in l. We approximate (B4) by log-linearization:

$$l_{i,t} \backsimeq l_i^* + \tilde{a}\{(\ln R_t - \ln R_i^*) - (\ln y - \ln y_i^*) + (\ln m_t - \ln m^*)\} + v_i,$$

where $\tilde{a} = \frac{R_i^* m^*}{g''(l^*) y^*}$ and $v_i = -\frac{g'(l_i^*)}{g''(l_i^*)}$. Noting that y is second-period base income and R_t is the person-specific interest rate, these are functions of household and individual characteristics $\mathbf{x}_{i,t}$ with $R_t = R(\mathbf{x}_{i,t})$ and $y = y(\mathbf{x}_{i,t})$. Further approximating these functions will give a linear equation in $\mathbf{x}_{i,t}$. Hence, we arrive at,

$$l_{i,t} - l_i^* \backsimeq \boldsymbol{\beta}'(\ln \mathbf{x}_{i,t} - \ln \mathbf{x}_i^*) + \gamma(\ln m_t - \ln m^*) + v_i, \tag{B5}$$

[17] These assure that $l^* > 0$ to exists. Given that almost everyone attends school to some level, and that the Government of Bangladesh introduced compulsory primary education in 1991, these conditions are an effective description of reality.

which provides the basis for the estimating equation (C6) in Section 3.

Passing the examination and continuing schooling are critical for students to achieve greater human capital for a future increase in income. The impact of having the annual final examination during the off-peak seasonal labor demand period is equivalent to a decrease in productivity or wage rates in this model. In Bangladesh in 1999, *Ramadan* school holidays resulted in rescheduling of the annual final exam of schools to the pre-harvest period. Hence, individuals faced a lower marginal labor productivity or wage during the examination period of 1999 than any typical year. This can be expressed as having a lower value for m. Comparing the favorable final exam schedule of 1999 with the unfavorable one of 2002 – between agricultural and non-agricultural households denoted with subscripts $_{ag}$ and $_{nonag}$ – will enable us to identify its impact on enrollment.

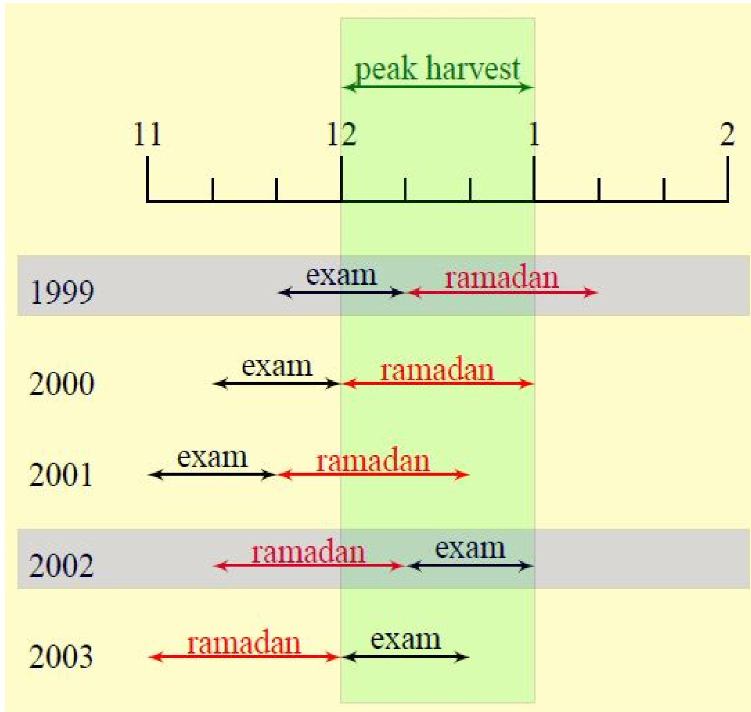
3 Identification and Estimation Strategy

3.1 Natural Experiment framework in Bangladesh

The challenge we face with the identification strategy for this study is threefold. First, there is no control group as *Ramadan* school holidays are a nationwide event. Therefore, we need to assess the differential exposure of seasonality among suitably defined groups, that is, children from agricultural (treatment) and non-agricultural households (control). Second, because we use the DID-FD specification, we must assess the likeliness of the common trend assumption, which is necessary for the consistency of estimates. We compare enrollment changes in the later waves of data – using 2002 and 2006 survey rounds – to verify whether we can observe common trends between the two groups. Third, because we utilize a natural experiment that lacks a fine control on events, we must assess the likeliness of other possible confounding factors that affect only agricultural households in the observed years. We address these points below.

As mentioned earlier, Bangladesh is a Muslim-majority country; thus, schools are in-

Figure 1: Sequence of Events.[a]



[a]Timing of annual final exams, *Ramadan*, and peak harvest period for *Aman* rice. *Ramadan* shifts by about 11-12 days each year. The starting dates of *Ramadan* in different years are the following: December 9th (1999), November 27th (2000), November 16th (2001), November 6th (2002) and October 27th (2003). Years in the shaded rows indicate the years compared in this study through the natural experimental framework.

structed by the Ministry of Education to provide holidays to facilitate *Ramadan*-related activities. In 1999, *Ramadan* was celebrated in December, as a result, schools had to move their final examinations forward to November. This created only a small overlap between the peak seasonal labor demand period for *Aman* rice and the final examination period. Three years later, in 2002, due to shifts in the lunar calendar dates on the Gregorian calendar, *Ramadan* was celebrated in November. Schools gave holidays in that month and scheduled the final examinations in December, which is the usual schedule that overlapped with the *Aman* harvest season.

This exogenous shift makes 1999 a favorable year for students – especially students from agricultural households – as the scheduled annual exam came ahead of the harvest season avoiding a conflict with seasonal labor demand. In contrast, 2002 becomes a candidate for a counterfactual of 1999. This paper uses 1999-2002 (collected in 2000 and 2003) longitudinal data-set to estimate the impact of the overlap between the peak labor demand and the examination period on the school continuation rate in Bangladesh between the agricultural (treatment) and non-agricultural (control) households.

Given that we are interested in the impacts of the off-harvest examination schedule (when seasonal labor demand is low) on enrollment, we must compare individual outcomes with a credible counterfactual. With the panel data in the absence of random variations affecting the magnitude of labor demand, the most credible strategy is to use the DID specification by assuming that one can observe a group of individuals who are exposed more to seasonal labor demand variability as compared with another group. We assume that children from agricultural households are exposed to the seasonal labor demand more than those from non-agricultural households; hence, in 1999, the children from agricultural households are expected to enjoy a relatively smaller reduction in school enrollment rates. This is based on the presumption that poor agricultural families typically engage their children with farming activities during harvest season, not only to save on the cost of harvesting, but also to reap the benefits of a greater seasonal labor demand and higher wages in the labor market. In addition to these supply-side justifications, one can also think of the demand-side preference for children from agricultural households since they have stronger ties with the agricultural community, have more seasonal agricultural-based job networks, and have more experience in agricultural activities, all of which make them more employable.[18]

The *Ramadan* timing variation as a natural experiment has been successfully applied in the economics literature. Almond and Mazumder (2011) employ *Ramadan* as a natural experiment for forcing smaller food intake. They control for its seasonality by exploiting the shifting nature of *Ramadan* that results from its determination according to the lunar calendar. Oosterbeek and van der Klaauw (2013) exploit the same shifting pattern of *Ramadan* over a five-year period as a source of differing exposure to fasting and estimate its impacts on the examination scores of Muslim graduate students in the Netherlands. Campante and

[18]We also note that non-agricultural households tend to face peak labor demand, if any, at different times of the year, rather than during the harvest season (e.g., during new year celebrations).

Yanagizawa-Drott (2013) examines the impacts of *Ramadan* fasting on the labor market and economy-wide outcomes.

In a natural experiment framework with considerable impact coverage among population, previous studies have focused on differences in shock exposure in identifying the parameters of interest. In further safeguarding against the risk of endogenous exposure, most of previous studies also used the difference-in-differences (DID) technique to eliminate unobservable fixed effects from estimating equations. For instance, Mansour and Rees (2012) use the exposure to battle intensity to estimate the impacts of wars on fetal growth. They employ sibling differences to eliminate the mother fixed effects. Fransen et al. (2015) estimate the impacts of forced migration on learning (growth), in which they use the distance from the border to capture differential exposure and take a time-difference to control for individual fixed effects. Oosterbeek and van der Klaauw (2013) take a time-difference to eliminate individual fixed effects, and compare Muslim and non-Muslim students who have different exposure to Ramadan fasting. Agarwal and Qian (2014) use national versus foreign residents as their treated and control sample, and take a time-difference to compare the impact of a massive subsidy program.

Our identification strategy is similar to the one used by Oosterbeek and van der Klaauw (2013); we take a time difference of the same individual to eliminate individual fixed effects; we use differences in exposure to harvest labor demand between households during exam periods to identify its impacts on school continuation. Our strategy uses an additional sensitivity check by examining the common time trend between the years not used for the estimation, 2002 and 2006.

FIGURE 1 presents the schematic explanation of the timing of key events. In 1999, the annual final examination period partially overlapped with the harvest period. In 2002, the examination and harvest took place concurrently after the *Ramadan* holidays. For the students who were preparing for the examinations, this implies that they were facing lower wage rates or smaller seasonal labor demand during the examination period of 1999 than during that of 2002. We utilize this variation to identify the impacts of lower labor demand during the examination period as demonstrated in Section 2.

A clarification on the confounding factors is needed to qualify our analysis and interpretations. In 1999, the examination was held before the harvest and before the *Ramadan* school holidays. In 2002, the examination was scheduled during the harvest and after the *Ramadan* school holidays. Therefore, in our natural experimental framework, there exist two potential "treatments", which are a) examination coinciding with vs. avoiding the harvest season, and b) examination before vs. after the *Ramadan* school holidays. Through-out our analysis, we emphasize on the effect of point (a) by comparing the impacts of the examination calendar shift on agricultural and non-agricultural households. For point (b), we assumed that holding the examination before or after school holidays affects students in both agricultural and non-agricultural households similarly.[19] However, it is possible that the home-learning environment during the time of festivity is different between agricultural and non-agricultural households, which could systematically affect exam preparation and school continuation. To check this formally, we compare Muslim and non-Muslim households in the sample. Since non-Muslims do not fast and are less prone to be affected by the festivities, this exercise helps us to disentangle the festivity impact, which is found to be statistically negligible, and our main findings remain unchanged. This also provides a check on the impact of fasting: non-Muslim students do not fast, so the statistically zero estimate for the non-Muslim dummy affirms that fasting is not the source of enrollment rate variations across households. More on this robustness check is described in the next subsection.

[19] Although it is true that in 2002, exams were not followed by a school break, which could have some impact on school continuation (for example, students got adequate opportunity to have proper rest which might improve academic continuation) but this is true for all the students from agricultural and non-agricultural households. There is no particular reason to believe that not having a school-break after the exam will systematically affect students from agricultural households to discontinue schooling.

3.2 Empirical Specification

When we consider the dynamic schooling choice as the following equation:[20]

$$y_{i,t} = \boldsymbol{\beta}'\mathbf{x}_{i,t} + \gamma r_t D_i + \delta r_t + \eta D_i + v_i + e_{i,t}, \tag{C6}$$

where $y_{i,t}$ is a binary variable indicating enrollment for an individual i at period t, $\mathbf{x}_{i,t}$ is a set of exogenous covariates, r_t is a dummy variable for the year 2002 (when the school exam schedule coincided with the harvest season), D_i is a dummy variable for agricultural households, v_i is the time-invariant individual effect, and $e_{i,t}$ is the error term.

In equation (C6) we estimate enrollment continuation with a binary variable $y_{i,t}$. As we do not observe labor demand nor wage rates during the examination period, we take $r_{i,t}D_i$ as a proxy of increased seasonal labor demand or increased opportunity costs of studying for children of agricultural households during the 2002 examination period.

The coefficient δ of the year 2002 dummy $r_{i,t}$ accounts for all other effects in 2002. γ is our main coefficient of interest that measures the enrollment change in 2002 from that in 1999 among agricultural households relative to non-agricultural households, which we attribute to an unfavorable school exam schedule that coincides with the seasonal labor demand of the harvest period. Under a dynamic setting, $\mathbf{x}_{i,t}$ includes all other relevant variables that affect future income and effective interest rates faced by individuals.[21]

As a general trend observed in the low-income areas, enrollment rates decrease as children progress in school. Thus, we control for the baseline dropout rates for each cohort. Assuming that individuals in the same cohort face the same dropout distribution given observable and individual fixed effects, we can expand the estimating equation to include the baseline

[20] See (B5) in Section 2.

[21] These are, in general, functions of parental characteristics and wealth levels. Therefore, we incorporate variables such as head-of-household education level and landholding. These variables may also affect home production and home education production processes, so the interpretation of their estimates can be either or all of future income, effective interest rate, current production inputs, and home education production inputs. As this is not our main focus, we do not attempt to derive a structural interpretation of these estimates.

dropout rates for the cohort c_i and observable baseline characteristics $\boldsymbol{\omega}_i$. Hence, the base model changes to the following:

$$y_{i,c,t} = \boldsymbol{\beta}'\mathbf{x}_{i,t} + (\gamma + \boldsymbol{\gamma}_\omega'\boldsymbol{\omega}_i + \boldsymbol{\gamma}_c'\boldsymbol{c}_i)r_{i,t}D_i + \delta r_{i,t} + \eta D_i + \boldsymbol{m}_c'\boldsymbol{c}_i + \boldsymbol{m}_\omega'\boldsymbol{\omega}_i + v_i + e_{i,c,t}, \tag{C7}$$

where a suffix $_c$ is an index for cohort c. We allow for cohort fixed effects m_c for all c and cohort-wise impacts $\boldsymbol{\gamma}_c$ on a cohort dummy vector $\boldsymbol{c}_i$ for i, where the j-th element in $\boldsymbol{c}_i$ takes the value of 1 if i belongs to cohort $c = j + 1$. We also allow the impacts to be correlated with baseline characteristics $\boldsymbol{\omega}_i$ through $\boldsymbol{\gamma}_\omega$.

In general, we expect $\mathbf{x}_{i,t}, D_i \not\perp v_i, \boldsymbol{c}_i, \boldsymbol{\omega}_i$. So we first-difference the equation (C7) to eliminate the fixed effects:

$$\Delta y_{i,c,t+1} = \boldsymbol{\beta}'\Delta\mathbf{x}_{i,c,t+1} + (\gamma + \boldsymbol{\gamma}_\omega'\boldsymbol{\omega}_i + \boldsymbol{\gamma}_c'\boldsymbol{c}_i)D_i + \delta + \Delta e_{i,c,t+1}. \tag{C8}$$

In (C8), we denoted $\Delta A_{i,c,t+1} = A_{i,c,t+1} - A_{i,c,t}$ for a given covariate A and used $r_{i,t} = 0, r_{i,t+1} = 1$. The remaining correlation between D_i and c_i, ω_i is assumed to be controlled and partly tested by using interaction terms. Hence our main coefficient of interest is γ which captures the DID estimates.

DID specification requires a common trend assumption in enrollment rates between agricultural and non-agricultural households. This cannot be shown with the same estimation data, but we can indicate suggestive evidence that it may hold. Our data set was collected in three rounds, with the final round collected in 2006.[22] The first and third rounds are separated by seven years; therefore, many children had already finished their schooling when the third round of data was collected. Thus, the third round data-set does not have much schooling information for those who were aged 10 years and older in the 1999 round, making the triple difference estimator infeasible. Nevertheless, we can still measure and compare the overall enrollment rates of agricultural and non-agricultural households across survey

[22] See `http://www.ifpri.org/dataset/chronic-poverty-and-long-term-impact-study-bangladesh`.

rounds. We need to test that no difference in enrollment rate trend exists between the 2002 and 2006 rounds (second and third rounds), which qualifies as a placebo test and provides a justification for our DID strategy.

We compared the mean enrollment rates among agricultural and non-agricultural households between 2002 and 2006 rounds of data set for 870 individuals aged 10-18 years in 2002, which is our age band for the estimation. We have assumed that attired individuals are not attending schools. We noticed that changes in enrollment rates are 29.7% for agricultural households and 25.1% for non-agricultural households. The proportions test for equal change in enrollment rates (i.e., testing the null of equal changes in enrollment rates) provides the p-value of 0.32.[23] This exercise indicates that the common trend assumption may hold in our sample.[24] In a separate exercise, we further tested the common trend in various sub-samples (cohort s) between 2002 and 2006. We also tested the equality in rates of change $\frac{\Delta y_t}{total_t}$ and rate differences $\frac{y_t}{total_t} - \frac{y_{t-1}}{total_{t-1}}$, where $total$ is the size of cohort s and confirmed that none of the results reject the null of common trend.[25]

3.3 Other Threats to Identification

We noted that there exists other potential threats for internally valid inferences with our natural experimental framework, which we describe below in detail.

First, even if γ is estimated with precision, agricultural households may share unobservable characteristics that result in a larger decrease in enrollment rates in 2002 as compared with non-agricultural households. As we are controlling for individual fixed effects, the remaining unobservable characteristics that we must consider are the time-varying ones. The

[23] Ideally the examination of the common trends assumption should also include grade progression. In our study, enrollment is decided with the year-end exam results, so enrollment is synonymous with grade progression in our data.

[24] When we restrict our sample by shifting to the older cohorts, we obtained more equal changes in enrollment rates; however, the power of test gets weaker as the sample size becomes smaller with older cohorts.

[25] We further tested common trend using age specific regressions, and found that the common trend cannot be rejected for all the age groups except for children who are 13 year-olds in 2002, which is a natural transitional age for students in Bangladesh. As a precautionary exercise and to check the robustness of our estimate, we dropped all 13 year-olds from our sample and re-estimated our main regression specification, which provided similar results as found in TABLE 3.

most likely candidate is the possibility of an incidentally large agricultural labor demand in 2002. Even if *Ramadan* in 1999 had no impact on enrollment, a good harvest in 2002 may have induced greater school discontinuation for agricultural households relative to non-agricultural households, resulting in a larger drop in enrollment rate. Hence one needs to include paddy productivity among the covariates. Unfortunately, the data-set we use focuses on schooling and pays sparse attention to production-related information. As a proxy for paddy production variability, we included district specific *Aman* paddy production information in our regressions, collected from the Bangladesh Bureau of Statistics (BBS). We note that the Bangladesh Bureau of Statistics (BBS) reported national production of *Aman* rice does not significantly differ between these two seasons (*Aman* season of 1999 and 2002) within our sample household districts, with 5,010 thousand metric tons produced in 1999 and 5,342 thousand metric tons in 2002, only a 6.6 percentage change in production.[26] In all regressions, we also additionally include the year 2002 dummy as well as its interaction terms with the location dummies,[27] which capture all other time-variant causes that could affect enrollment that are common at the *thana* level (e.g., seasonal flood in some riverine sub-districts which could also have an impact on enrollment). Additionally we control for temperature variation at the local level, which can simultaneously influence school continuation as well as agricultural productivity and income of our sample households.[28] Table 11 reports all these additional controls.

Second, it is arguable that our identification strategy cannot separately identify *Ramadan* impacts from any event peculiar to 2002 that is unrelated to productivity shocks, such as having holidays before the examination period. This occurred in 2002, but not in 1999. This

[26]If wage elasticity of yield is ψ and labor supply elasticity of wage is ξ, impacts on labor supply in the 30-day harvest period is $0.06 \times \psi \times \xi \times 30 = 1.8\psi\xi$. Even if we assume relatively large elasticities $\psi = 1.5$, $\xi = 1$, we have only 5.4 percent increase in labor supply per-day during the two-month long harvesting period in 2002. We assume that this magnitude will not change the passing rate for the final examination.

[27]Thana is the lowest administrative unit at the sub-district level.

[28]weather data is taken from Bangladesh Meteorological Association monthly data at the district level. We used annual level of mean high and low temperature in our regressions. Please note that we could not use rainfall data as the data was highly collinear with the paddy production data.

can hamper learning for children whose home-study environment is unfavorable.[29] Therefore, it is possible that our interaction term between the agricultural household and the year 2002 dummy may be detecting the impacts of having long holidays before exams that are specific to agricultural households.

To tackle this concern, we include parental education variables to the covariates. We assume that parental education is correlated with the quality of the home learning environment, as more educated parents with personal schooling experience would be aware of the most suitable learning environment for children when an examination is near. As maternal education can play a key role in home learning (Behrman et al., 1999), we include both parents' education variables in our regressions.

As *Ramadan* is synonymous with daytime fasting, it should draw attention that fasting before the exam in 2002 may have caused the children in agricultural households to underperform academically, as they are presumed to be more constrained on food availability. Similarly, there may be adverse impacts from the celebration of *Eid-ul-Fitr*, one of the major festival for Muslims, including in Bangladesh, which marks the completion of the 30 days of fasting of *Ramadan*. The impact of *Eid-ul-Fitr*, which was observed before the annual exam in 2002, might have affected children's annual final examination preparation through unfavorable study environment at home (long festive mood) or through lack of food supply during the exam period after the celebration (post-festival effect before the harvesting) due to overspending or unplanned consumption during the *Eid-ul-Fitr*.[30]

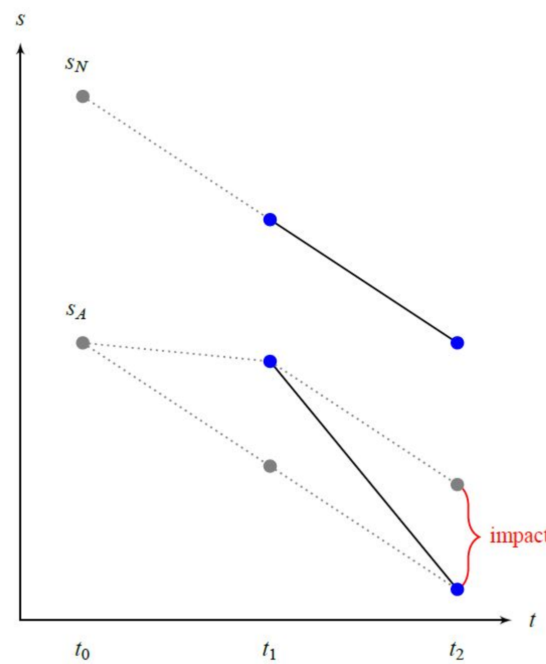
To consider this impact, we compare Muslims and non-Muslims where the children from the latter should not be affected from fasting or other adverse impacts of the *Eid-ul-Fitr*. Although Bangladesh is predominantly a Muslim country, there exists a non-negligible number of non-Muslims. We use a non-Muslim religion dummy variable interacted with our

[29]The home learning environment during the festival could be poorer due to many reasons. For example, having a large family get-together during the festival time could affect the study environment even after we control for observable wealth measures such as landholding, non-land assets, and official poverty status.

[30]However, there is no particular reason to observe adverse impact of festivity between agricultural households and non-agricultural ones.

main variable of interest $r_{i,t}D_i$ to see differential impacts. Due to the small sample size of non-Muslim households, the power of this exercise is lower than that of the other tests we conduct; however, we believe it offers additional robustness checks for estimation and interpretation. Including the non-Muslim dummy and its interaction with agricultural household does not alter $\hat{\gamma}$, our main coefficient of interest, which suggests that such a concern may not be valid for our study.

Figure 2: Graphical illustration of identification.[a]



[a]In the diagram s denotes the enrollment rate and t denotes time. Gray points are not observed but assumed. Blue points are observed in our experimental framework. We assume a common trend in the absence of *Ramadan* induced school holidays, and it's impacts keep students at school in t_1 who would have otherwise dropped out. Such favorable impact dissipates in t_2.

3.4 Explanations of DID Framework

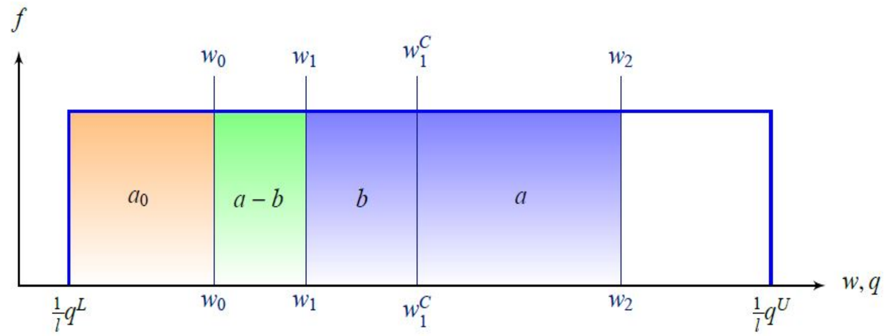
We note that our natural experiment under a DID framework uses the opposite time direction than the regular case where the policy or intervention happens between the first and second rounds. In our case, the exogenous shift of favorable exam schedule happened during the first round of the survey. This uplifting impact allowed more children from agricultural households to remain in school in 1999. As shown in FIGURE 1, this uplifting impact continued until 2001, and, in 2002, the typical concurrence of harvest and exam period kicked in.

The reverse time order of policy and observation requires an additional assumption from regular DID specification. In addition to the common (parallel) trend in the absence of an exam schedule shift, one needs the impact to be one-time and tapering off by the second period. FIGURE 2 graphically illustrates these two assumptions. For the ease of exposition, we include the before-baseline time period t_0. The *Ramadan* favorable examination schedule happens in period of t_1 and disappears in period t_2. In the absence of this exogenous shift in exam schedule at period t_1, both agricultural and non-agricultural households should have shared a common trend (depicted with dotted gray lines in Figure 2). With the aid of a favorable exam calendar in t_1, the observed enrollment rate of agricultural households (depicted with solid line in Figure 2) is higher than the counterfactual. However, once it reaches period t_2, with the typical concurrence of the final examination coinciding with harvest season, we see a disproportionate decrease in the enrollment rate for students in agricultural households relative to students in non-agricultural households. This disproportionate decrease is what we aim to estimate as impacts of an unfavorable examination calendar that coincides with peak harvest season.

One may feel that the one-time impact assumption is too strong to be justified. One can rationalize this by reinterpreting the estimates in the spirit of local average treatment effect (LATE) estimates. If we consider the interaction of the natural experiment and agricultural household dummy as an instrumental variable, then the estimated impacts are defined on the subpopulation of agricultural households that discontinues schooling when exam and seasonal labor demand coincide. This subpopulation of agricultural households will discontinue schooling as soon as the favorable examination schedule ends, so one can consider the impacts to be a one-time event. Rather than assuming that the impacts are once-off, we prefer to interpret the estimates similar to LATE estimates, defined on the subpopulation.

The economic intuition behind our identification can be illustrated with a simple wage

Figure 3: Graphical illustration of dropout.[a]



[a]Productivity q is assumed to be distributed uniformly. Wage is assumed to evolve constantly per period, and impacts of non-concurrence is assumed to be short lived. w_t is period t wage rate and w_1^C is the counterfactual wage rate when examination and harvest coincide.

distribution. In our theoretical framework in the Section 2, it is shown that FOCs can be written as a function of wage w:

$$l^* = l(w), \quad l' < 0,$$

where we dropped interest rate R and second-period income y for brevity. Suppose, for simplicity, that there exists a threshold level of study hours $\bar{l}$ such that $l^* < \bar{l}$ is so small that students makes a discrete decision to drop out. Then

$$\begin{cases} \text{study} \\ \text{work} \end{cases} \quad \text{iff} \quad l(w) \gtreqless \bar{l}.$$

Assume that the marginal returns to schooling $e'(l)$ is heterogenous among students and has the functional form of $e'(l) = \frac{q_i}{l_i}$. Consider that $\frac{q_i}{l_i}$ is uniformly distributed $\frac{q_i}{l_i} \in [q^L, q^U] \subset \mathbb{R}_{++}$. Then the optimal schooling by the student i is given by $l_i = q_i \frac{1}{w}$. The above can be rewritten as

$$\text{work} \quad \text{if} \quad \frac{q_i}{w} \leqslant \bar{l} \quad \Leftrightarrow \quad \frac{q_i}{\bar{l}} \leqslant w.$$

Suppose further that wage w evolves through time $t = 0, 1, 2,$ as

$$w_t = w_0 + g_1 t - g_2 r_t D, \quad g_1, g_2 > 0,$$

where $r_t = 1$ when examination and seasonal labor demand do not coincide, 0 otherwise, and $D = 1$ for agricultural households, 0 otherwise. In FIGURE 3, we show students are distributed in a wage interval of $\left[\frac{q^L}{\bar{l}}, \frac{q^U}{\bar{l}}\right]$ and all students located to the left of current wage will drop out.

Note that the evolution of wage assumes an increasing trend and that g_2, the uplifting impact, is short lived and does not carry over to the next period.[31] Consider a counterfactual that examination and seasonal labor demand coincide in period 1. Then, child wage for agricultural households evolves as

$$w_t = \begin{cases} w_1^C \\ w_2 \end{cases} = \begin{cases} w_0 + g_1, \\ w_0 + 2g_1, \end{cases}$$

where w_1^C stands for counterfactual period 1 wage, had the examination and harvest coincided. In reality, examination and harvest did not coincide in $t = 1$, so the wage rate becomes $w_1 = w_0 + g_1 - g_2$.

When we apply these wage rates and schooling choices to the diagram in FIGURE 3, we can see that an additional $a = \frac{g_1/\bar{l}}{q^U/\bar{l} - q^L/\bar{l}} = \frac{g_1}{q^U - q^L}$ percent of students drop out per period when examination and harvest coincide.[32] In period 1, lack of this concurrence prevents $b = \frac{g_2}{q^U - q^L}$ percent of students from discontinuing schooling. In the next period, however, this favorable condition is lost and $a + b = \frac{g_1 + g_2}{q^U - q^L}$ percent of students drop out.

We note the complementarity of our identification strategy with a conventional, more structural approach that treats the time allocation decisions of children more explicitly. In

[31] This wage increase is arguably arbitrary yet the rising opportunity cost is a general consequence of growing up.

[32] In period 0, the diagram assumes that a_0 percent of students are already out of school.

theory, our investigation can be conducted with often-used exogenous random variations such as rainfall recorded at the closest weather stations, possibly interacted with household characteristics, to measure the observed productivity or its changes on children's labor supply. One then applies the estimated parameter in determining attendance, for example, as a residual time use after labor has been supplied, relying on an assumption of mutual exclusivity of work and school. Having rainfall data alone helps in constructing the counterfactual with more precision if it is correlated with the strength of labor demand, although it will not allow structural parameters to be estimated. The structural approach requires rainfall and labor supply data which we do not have, and an additional assumption of mutual exclusivity. Knowledge of labor supply elasticity might facilitate hypothetical policy exercises, but it comes at the cost of using additional data and an assumption of mutual exclusivity to compute the drop out elasticity. Compared to this, our identification strategy and DID set-up provides a straight-forward solution to the research question.[33]

Our identification strategy employs a natural experiment framework that shifted the exam ahead of harvest season. In our DID framework, we control for time-invariant individual characteristics, time-variant aggregate unobservables, time-variant geographical (thana)-level unobservables (controlling issues such as rainfall variations), and cohort trends. We also test for common trends in enrollment rates among agricultural and non-agricultural households between 2002 and 2006 for the same age groups. In addition, we control for district level *Aman* rice production and sub-district level weather variation in our regression. Moreover, we checked the religion-specific effect by including non-Muslim samples in our estimations. However, one may be concerned about whether there exists any particular issue

[33]In addition, in the absence of explicit randomization, researchers often lack valid and sufficiently strong measures for productivity. Rainfall, a candidate measure for a more structural approach, may be weakly correlated with raw plot productivity, because rainfall is often recorded at weather stations that cover spatially wide areas. There is an inherent "basis risk" in the use of recorded rainfall as an instrument for productivity if geographical information is not available. In addition, the same rainfall may have differing impacts on yield across plots, considering geographical characteristics such as elevation, canal/drainage accessibility, and underground water flows, even after controlling for observable differences in farmer characteristics.Moreover, readily available altitude information that can be found in using a geographic information system (GIS) can include large measurement errors.

at the individual level in 2002 relative to 1999 that systematically prompted individuals to drop out from only agricultural households (e.g., household-level productivity shocks that are uncorrelated with aggregate productivity shocks), for which we do not have sufficient data to control for. Except for this, we control for a wide range of factors that could affect the enrollment variation of our estimation, which shows the extent of credibility that our analysis conveys.

4 Data, Definitions, and Descriptive Statistics

The data set we use is a panel-data collected in 1999, 2002 and 2006 in rural Bangladesh by the International Food Policy Research Institute (IFPRI). It surveyed 600 households from 60 villages in 30 unions to investigate the impacts of Food for Education (FFE) programs on school enrollment. From these households, 2,597 individuals were surveyed. In our main DID analysis we used the balanced portion of the 1999-2002 survey data with age cut-off of 10 to 18 years old in 1999, consisting of 682 individual observations. For placebo regressions we used the balanced portion of the 2002-2006 data, with the same age cut-off of 10-18 years old in 2002, which gave the placebo sample 870 observations. For detailed description of the data cleaning process and descriptive statistics of variables used in the main and placebo estimation, please refer to Appendix A1.[34]

In our main DID analysis, we compare the enrollment trends of agricultural and non-agricultural households of 1999 and 2002, respectively.[35] To define an agricultural household we considered a broad range of definitions that regarded a household as agricultural if any

[34]The 1999 and 2002 data sets are known as the *Impact Evaluation of Food for Education Program in Bangladesh 2000*, and *Comparing Food versus Cash for Education program in Bangladesh 2003* data set, respectively. The 2016 round data-set is known as *Chronic poverty and long term impact study in Bangladesh*. For more information, see `https://www.ifpri.org/publication/impact-evaluation-food-education-program-bangladesh-2000`, `https://www.ifpri.org/publication/comparing-food-versus-cash-education-program-bangladesh-2003`, and `https://www.ifpri.org/publication/chronic-poverty-and-long-term-impact-study-bangladesh`.

[35]Here enrollment information confirms the school continuation of each student as discontinued student will not enroll at a school at the beginning of the academic year. Schools typically confirm this by the middle of the academic year in Bangladesh, when they are obligated to report this to the local administration and to the education ministry.

household member reports his or her main income source or main occupation as agriculture or if a household owns agricultural plots. We also used an alternative definition of agricultural household whereby the household head reports his or her main income source or self-reported occupation as agriculture. Nevertheless, the different definitions are highly correlated with each other and estimated results are similar, as reported in the next section.

Given that agricultural and non-agricultural households engage in different types of economic activities, we expect their characteristics to differ. We compare their means and tested for their differences, as shown in Table 1. Agricultural households have 7.4 percentage points (pp) less access to electricity, live further from the nearest school, have a larger household size by 0.7 persons, have more structured toilets by 9 pp, and have more land per person by 8 decimals. Heads of households and their spouses tend to be older and less educated in agricultural households. However, between agricultural and non-agricultural households, safety net or education related program (stipend/scholarship) support, per capita consumption and non-land asset holding, official poverty status, and water access are similar. These are considered to be correlated with being credit constrained and the ability to cope with economic shocks, thereby affecting the opportunity costs of schooling. It shows that two groups we compare are expected to be similar in these unobservable characteristics. We also note there are characteristics with differences below 5 percent cutoff in p-values. We include them in the estimation as covariates to control for their potential impacts on schooling.

We select our sample cohorts from the 1999 data set and set the lower age cutoff at 10 years old, based on the definition of child labor used in the Labor Force Survey (LFS) of Bangladesh, capturing both primary and secondary school enrolled students.[36] We also use a different sample with different age cutoff (11 to 18 years old) for robustness checks. The data set includes reasons for discontinued schooling, which are presented in Table 14 in the Appendix. We see that poorer households cite financial difficulties as the main cause for their children dropping out from school, while the wealthier households cite non-financial reasons,

[36]Compulsory school enrollment for primary education in Bangladesh is from age 6-10. For more information, see `http://uis.unesco.org/en/country/bd`.

such as marriage. We also find that agricultural households cite financial reasons more frequently than non-agricultural ones for their main reason for school dropouts. These results suggest that we need to control for household wealth in analyzing school enrollment decisions. We include per-capita land and non-land asset holding as covariates in our regressions.

As Bangladesh is known for its proactive safety net and educational aid policies, one must consider these support programs in analyzing enrollment behavior. However, it is difficult to assess causality, as we cannot control for the actual targeting rules used by various institutions for different supports; however, this indicates the necessity of controlling for educational aid and safety-net access in the estimation.

Table 1: **Characteristics of Agricultural and Non-agricultural Households**

Variables	Agricultural households	Non-Agricultural households	p-value
Access to electricity	0.108	0.182	0.031
Access to piped water	0.391	0.345	0.060
Access to structured toilet	0.329	0.240	0.000
GPS based distance to school	0.556	0.468	0.028
Household size	6.639	5.939	0.000
Per capita non-land asset (1000 taka)	10.421	11.480	0.337
Per capita land holding (decimal)	21.459	13.246	0.007
Dummy for stipend/scholarship	0.713	0.760	0.027
Age	13.061	12.942	0.521
Sex (Female = 1)	0.489	0.542	0.037
Head age	49.539	44.917	0.000
Head sex (Female = 1)	0.037	0.254	0.000
Head primary	0.349	0.343	0.880
Head secondary	0.171	0.265	0.020
Spouse age	40.189	37.373	0.001
Spouse sex (Female = 1)	0.992	1.000	0.158
Spouse primary	0.354	0.261	0.056
Spouse secondary	0.280	0.388	0.034
No. of Observations	407	275	682

Source: Compiled from IFPRI data set.

5 DID regressions

5.1 Main Estimates

Table 2 shows the estimates of our main coefficient of interest $\hat{\gamma}$ for the sample with the lower age cutoff age of 10 -18 years old in 1999. In the first three columns of Table 2 we reported regressions with the broadest definition of agricultural households, as described in the previous section. In the next two columns we reported regression estimates with narrower definition of agricultural households (termed as "Agricultural (Head)"). In the last two columns we restricted sample to sons and daughters of household heads (termed as "nuclear members"). In Table 3 the same estimates are repeated for robustness checks with higher age-cutoff of 11-18 years old in 1999.

In Table 2, column (1) reports the most basic DID specification, for which we use only the year 2002 dummy and its interaction term with the agricultural household. Our estimation shows that the impact of annual final exam coinciding with seasonal labor demand has a 7.2 pp negative impact on the enrollment rates of students from agricultural households (from the base of 45.8 percentage enrollment of non-agricultural households' children). Similar estimations with higher age cutoffs are reported in column (1) of table 3 showing higher magnitude of impact for older children. These estimates suggest that in 2002, the enrollment rates of children from agricultural households dipped more severely than of those from non-agricultural households, attributed to the conflicting academic and agricultural calendar.

In Column (2) of Table 2, we introduce other time variant controls (such as district level yearly weather and paddy yield variations as well as quadratic terms of child's birth cohort to capture natural change in education over time) along with individual and household level controls (such as sex of the child and household asset). Additionally we control for household level hygiene condition — captured through access to piped water and structured toilet — that can affect health and school continuation. Moreover we added parental education

Table 2: **Main regression estimates with 10-18 years old**

Dependent Variable:	**Agricultural Household**			**Agriculture (Head)**		**Nuclear Members**	
Change in Enrollment	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year 2002 dummy interacted with Agricultural household	-0.072**	-0.068**	-0.065***	-0.072***	-0.070**	-0.084***	-0.078***
	(0.032)	(0.028)	(0.029)	(0.025)	(0.028)	(0.028)	(0.032)
Other Household level Control	No	Yes	Yes	Yes	Yes	Yes	Yes
Sub-district (Thana) Control	No	No	Yes	No	Yes	No	Yes
Number of Observations	682	682	682	682	682	632	632
R-Square	0.0040	0.4464	0.4456	0.4471	0.4464	0.4598	0.4582
Mean of control group in 1999	0.728	0.728	0.728	0.76	0.76	0.77	0.77
Mean of control group in 2002	0.458	0.458	0.458	0.48	0.48	0.5	0.5

Source: Compiled from IFPRI data. Cohort of 10 - 18 year aged children in 1999. Notes: 1. Regression estimated using a first-difference estimator with standard errors clustered at *thana* level. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for time variant covariates: yield which represents Thana level paddy yield, program which is an indicator variable if at least one household member receive either safety net or education related program (stipend/scholarship) support, mean high temperature is mean annual temperature of the daily high and mean low temperature is mean annual temperature of the daily low. 3. Time invariant variables are also interacted with year 1999: agricultural household is an indicator variable for a household whose primary income is agriculture. sex (female = 1) is an indicator variable of child gender. head primary, head secondary, spouse primary, spouse secondary are indicator variables for the respective highest educational achievement. per member land holding is per member land holding of the household in 1999 measured in acres. per member non-land asset is per member non-land asset values in 1999 measured in 1000 BDT. own piped water, structured toilet are indicator variables of household ownership of each facilities. All dummy variables are demeaned. Number of sample is per year cross sectional units.

variables, to capture for home learning environment of the household, as discussed earlier.[37]

All these time-invariant variables are interacted with the year 2002 dummy in the regression. Despite having these additional interaction terms our main co-efficient of interest shows a similar level of impact (reported in column (2) in Table 2) with the same level of significance as found in Column (1). This finding is also strongly consistent with different agricultural household definitions (reported in column (4) in Table 2) or limiting households to nuclear members (reported in column (6) in Table 2). Our estimates are robust even using higher age-cutoff reported in column (2), (4) and (6) of Table 3.

Examining covariates (not reported due to brevity) reveals that having any safety-net support or education related aid have a large positive impact on enrollment. Per capita landholding interacted with year 2000 and agricultural household dummy shows negative estimates, indicating detrimental impacts of family labor demand due to, in our interpre-

[37]This is an important control for any event peculiar to 2002 that is unrelated to productivity shocks, such as having holidays prior to the examination period.

Table 3: **Main regression estimates with 11-18 years old**

Dependent Variable:	**Agricultural Household**			**Agriculture (Head)**		**Nuclear Members**	
Change in Enrollment	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year 2002 dummy interacted with Agricultural household	-0.101***	-0.074**	-0.075**	-0.078**	-0.080**	-0.081**	-0.076**
	(0.03)	(0.032)	(0.032)	(0.033)	(0.034)	(0.032)	(0.032)
Other Household level Control	No	Yes	Yes	Yes	Yes	Yes	Yes
Sub-district (Thana) Control	No	No	Yes	No	Yes	No	Yes
Number of Observations	557	557	557	557	557	520	520
R-Square	0.0087	0.4964	0.4980	0.4971	0.4988	0.5046	0.5068
Mean of control group in 1999	0.691	0.691	0.691	0.691	0.691	0.76	0.769
Mean of control group in 2002	0.386	0.386	0.386	0.386	0.386	0.48	0.5

Source: Compiled from IFPRI data. Cohort of 11 - 18 year aged children in 1999. Notes: 1. Regression estimated using a first-difference estimator with standard errors clustered at *thana* level. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for time variant covariates: yield which represents Thana level paddy yield, program which is an indicator variable if at least one household member receive either safety net or education related program (stipend/scholarship) support, mean high temperature is mean annual temperature of the daily high and mean low temperature is mean annual temperature of the daily low. 3. Time invariant variables are also interacted with year 1999: agricultural household is an indicator variable for a household whose primary income is agriculture. sex (female = 1) is an indicator variable of child gender. head primary, head secondary, spouse primary, spouse secondary are indicator variables for the respective highest educational achievement. per member land holding is per member land holding of the household in 1999 measured in acres. per member non-land asset is per member non-land asset values in 1999 measured in 1000 BDT. own piped water, structured toilet are indicator variables of household ownership of each facilities. All dummy variables are demeaned. Number of sample is per year cross sectional units.

tation, imperfect substitutability between family and hired labor. We also use per capita non-land asset value interacted with the year 2002 and agricultural household dummy variables. This interaction term (per capita non-land asset value interacted with year 2002 and agricultural household) is large, positive, and statistically significant (small p-value). This demonstrates the wealth effect and confirms that poorer agricultural households felt the impact more strongly. This is consistent with the cash-constrained interpretation of child labor. Estimates show that girls have lower enrollment prospects in rural Bangladesh. This is possibly because, in 1999 and 2002, employment opportunities requiring higher education were limited except in a narrowly defined garment industry, which is usually based in urban and semi-urban areas (Paul-Majumder and Begum, 2000; World-Bank, 2008)). Furthermore, in a patrilineal society such as rural Bangladesh, parents may consider it financially unrewarding to invest in their daughters education, because they will get married and not provide as much old-age support as sons (Chowdhury and Bairag, 1990; Fraser, 2010).

As discussed in the Section 3, one issue may remain in our identification strategy which is worth further consideration. It is possible that we may be detecting impacts of time-varying productivity shocks (for example due to weather variation) that may have increased labor demand in 2002. Although the aggregate production of *Aman* rice is not significantly different between the two waves of data, regional level variation may exist between these two rounds. To optimally control productivity differences, we also use the year 2002 dummy for aggregate productivity shocks and the thana and year 2002 interaction terms for time-variant thana-level productivity shocks (controlling for thana level fixed effect) as regressors, reported in Column (3), (5) and (7) of Table 2, which is our richest specification. As one can notice, inclusion of these interaction terms does not affect our main coefficient of interest. In all specifications $\hat{\gamma}$ has the low p-values and showed the expected sign. From Column (1) to Column (7) the point estimates of the year 2002 dummy with agricultural households range from 6.5 pp to 8.4 pp, and all estimates have p-values below 5% or 1%. Our estimates show robustness even using higher age-cutoff reported in column (3), (5) and (7) of Table 3. It is also noteworthy that higher age cut-off has larger point estimates.[38]

5.2 Placebo Tests

In Table 4 we presented 3 sets of placebo tests — with younger cohorts (4 - 9 years old), same-age group and same individual with later rounds of data. These are all groups that are not expected to be exposed to the favorable shifts in the final exam schedules of 1999. For instance, 4 - 9 years old in 1999 (younger cohort) were too young to be affected in 1999. Similarly, same age groups (10-18 and 11-18) and the same individuals in 2002-2006 panel are not affected, simply because there was not a shift in the final exam schedule during this period. These three groups present good opportunities to conduct placebo tests for our

[38]In addition, we use other covariates suggested by the theoretical model, such as poverty status, GPS-measured distance to schools, and anthropometric measures. Our findings are consistent even with the inclusion of these additional controls. However, all of these variables have large p-values and including these variables reduces the sample size drastically. As a result, we have decided not to report these estimates for the sake of brevity; they are available upon request.

analysis.

In column (1) we estimated the equation (C8) with 1999-2002 data for age cut-off of 4-9 year olds in 1999. This age cohort was relatively younger in 1999 and many of them where not even enrolled in school. As a result, this cohort had not benefited much from the favorable examination calendar of 1999. Hence children from agricultural households within this age cohort should not face any differential impact in 2002 due to the annual final examination overlapping with harvesting season. As hypothesized, results reported in column (1) show a smaller point estimate with large p-values, supporting our first placebo test.

In the second set of placebo tests, we estimated the equation (C8) with 2002-2006 data, which also provide the validity checks for the parallel trend assumption. In this placebo test, we use the same-age group (10-18 years old) as our main regression similar with age cut-off of 10-18 year olds in 2002. Column (2) of Table 4 reports the richest specification following the estimates of column (5) of Table 2. Similarly, column (3) of Table 4 reports regression the higher age cut-off of 11-18 years old, as reported in column (5) of Table 3. Both of these regressions show large p-values for impact of year 2006 interacted with agricultural households, validating our identification and providing essential data support for parallel trend assumption under the DID setting.

In Column (4) we reported the third placebo test with the same individuals (10-18 years old in 1999) with the later rounds of data. Since there was no shift in the academic calendar between 2002 and 2006, we do not expect any differential impact of children from agricultural households within these years. As we can see, our coefficient of interest $\hat{\gamma}$ has a smaller point estimates relative to main results with bigger standard errors, leading to a large p-value.

5.3 Other competing mechanisms

In Table 5 we reported two tests that check the plausibility of competing mechanisms that are consistent with the estimated results — one with non-Muslim and the other with flood.

Table 4: **Placebo Tests 1**

Estimations:	**Placebo Regression 1**	**Placebo Regression 2**		**Placebo Regression 3**
Panel-Data:	**1999-2002**	**2002-2006**		**2002-2006**
Age Cut-off:	4-9 in 1999	10-18 in 2002	11-18 in 2002	10-18 in 1999
Dependent Variable: ΔEnrollment	(1)	(2)	(3)	(4)
Year 2002 dummy interacted with Agricultural household	-0.062 (0.063)			
Year 2006 dummy interacted with Agricultural household		-0.010 (0.028)	-0.008 (0.017)	0.027 (0.019)
Other Household level Control	Yes	Yes	Yes	Yes
Sub-district (Thana) Control	Yes	Yes	Yes	Yes
Number of Observations	265	870	764	635
R-Square	0.1047	0.1871	0.2079	0.2553
Mean of control group in 1999	0.897			
Mean of control group in 2002	0.795	0.674	0.636	0.508
Mean of control group in 2006		0.434	0.386	0.283

Source: Compiled from IFPRI data. Notes: 1. Regression estimated using a first-difference estimator with standard errors clustered at *thana* level. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for time variant covariates: yield which represents Thana level paddy yield, program which is an indicator variable if at least one household member receive either safety net or education related program (stipend/scholarship) support, mean high temperature is mean annual temperature of the daily high and mean low temperature is mean annual temperature of the daily low. 3. Time invariant variables are also interacted with year 1999: agricultural household is an indicator variable for a household whose primary income is agriculture. sex (female = 1) is an indicator variable of child gender. head primary, head secondary, spouse primary, spouse secondary are indicator variables for the respective highest educational achievement. per member land holding is per member land holding of the household in 1999 measured in acres. per member non-land asset is per member non-land asset values in 1999 measured in 1000 BDT. own piped water, structured toilet are indicator variables of household ownership of each facilities. All dummy variables are demeaned. Number of sample is per year cross sectional units.

With the non-Muslim based tests, we want to check whether our estimates are capturing mainly the impacts of fasting and festivities, which may diminish children's capacity to learn and pass the exams. If this is true then it should not affect non-Muslim agricultural households, which do not participate in the fasting and festivities during *Ramadan* and we should get statistically significant positive estimates for the interaction term. In column (1) and (2) of Table 5 we reported the interaction of the year 2002, agricultural household, and non-Muslim dummies under different age cut-offs. The point estimates are negative and are statistically indistinguishable from zero. Our main coefficient of interest is not affected by the inclusion of these non-Muslim interaction terms. This underscores that fasting may not be the driver behind the higher dropout rate among children from agricultural households.

Another possible mechanism that can explain the estimated results is impacts of a natural

Table 5: **Competing Mechanisms Tests**

Estimations:	**Regression with non-Muslim**		**Regression with Flood**	
Panel-Data:	**1999-2002**		**1999-2002**	
Age Cut-off:	10-18 in 1999	11-18 in 1999	10-18 in 1999	11-18 in 1999
Dependent Variable: ΔEnrollment	(1)	(2)	(3)	(4)
Year 2002 dummy interacted with Agricultural household	-0.071**	-0.081**	-0.068**	-0.076**
	(0.030)	(0.034)	(0.029)	(0.034)
Year 2002 dummy interacted with Agricultural and non-muslim household	-0.083	-0.079		
	(0.062)	(0.084)		
Year 2002 dummy interacted with Agricultural household with flood areas			-0.016	-0.016
			(0.059)	(0.043)
Other Household level Control	Yes	Yes	Yes	Yes
Sub-district (Thana) Control	Yes	Yes	Yes	Yes
Number of Observations	682	557	682	557
R-Square	0.4463	0.4985	0.4435	0.4914
Mean of control group in 1999	0.73	0.69	0.73	0.69
Mean of control group in 2002	0.44	0.38	0.44	0.38

Source: Compiled from IFPRI data. Notes: 1. Regression estimated using a first-difference estimator with standard errors clustered at *thana* level. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for time variant covariates: yield which represents Thana level paddy yield, program which is an indicator variable if at least one household member receive either safety net or education related program (stipend/scholarship) support, mean high temperature is mean annual temperature of the daily high and mean low temperature is mean annual temperature of the daily low. 3. Time invariant variables are also interacted with year 1999: agricultural household is an indicator variable for a household whose primary income is agriculture. sex (female = 1) is an indicator variable of child gender. head primary, head secondary, spouse primary, spouse secondary are indicator variables for the respective highest educational achievement. per member land holding is per member land holding of the household in 1999 measured in acres. per member non-land asset is per member non-land asset values in 1999 measured in 1000 BDT. own piped water, structured toilet are indicator variables of household ownership of each facilities. All dummy variables are demeaned. Number of sample is per year cross sectional units.

disaster that systematically affected the agricultural households. If that is true then our estimates are capturing the impacts of natural disaster on school dropout. Indeed in 2002, there are a few districts which were affected by monsoon flash floods.[39] To check this formally, we reported the interaction term of flood (indicator variable for sub-districts those are affected by flood), year 2002 and agricultural household dummies with different age cut-off in column (3) and (4) in Table 5. As we can notice, including this interaction term does not affect our main estimates and the interaction term has a large p-value.

[39] Flood affected districts were Chandpur, Sherpur and Tangail. To know more about the flood monsoon based flash flood, please check the following website `https://reliefweb.int/report/bangladesh/bangladesh-monsoon-floods-2004-post-flood-needs-assessment-summary-report`

6 Long-term Cohort analysis in Bangladesh

Since the favorable examination calendar shift in 1999 was a country-wide event, which benefited the children from agricultural households for three years (from 1999 to 2001, see Figure 1) we should expect to see an overall rise in years of schooling for the affected cohort, nationally. To check this formally, we use the latest round of Household Income and Expenditure survey (HIES) 2016-17, officially known as HIES 2016.[40]

We utilized the HIES 2016 to create birth cohort and years of education to conduct our analysis.[41] The estimation exploits year of birth as the identification. Here we assumed that parents did not make their fertility decision based on the favorable examination calendar in 1999-2001. Given HIES does not have the parental occupation information of the household, the estimation assumes that the rural population has a greater ratio of agricultural households than the urban population, and uses the rural population as its proxy. Under this setting, we expect that the rural population of the cohort 10-18 in 1999 has a longer years of schooling relative to its urban counterpart. This necessarily subsumes measurement errors in estimates. We use a proxy variable of agricultural households with the rural population dummy. Therefore, we interpret the results as attenuated from the actual impacts and finding the impacts adds to the plausibility of favorable effects of final exam rescheduling in 1999.

Formally we estimate the following equation for individual i, living in region j belongs to cohort t:

$$Y_{i,j,t} = \boldsymbol{\omega}_1 \text{Cohort}_t + \boldsymbol{\omega}_2 \text{Rural}_j + \boldsymbol{\omega}_3 \text{Cohort}_t \times \text{Rural}_j + V_j + e_{i,j,t}, \tag{F9}$$

where $Y_{i,j,t}$ is the outcome variable of interest. $X_{i,j,t}$ is a set of control variables (age

[40]HIES 2016 is a nationally representative household survey conducted by the Bangladesh Bureau of Statistics with a sample size of about 46000 households. To know more about this survey, please check the following link `https://catalog.ihsn.org/index.php/catalog/7399/study-description`

[41]Since our cohort of interest is 10-18 years old in 1999, we could not use earlier rounds of HIES surveys for our analysis as many of these age-cohort are still continuing education.

dummy capturing both demand and supply side change in education over time, sex, and religion). $\boldsymbol{\omega}_1$ is indicates whether an individual belongs to a particular birth-age cohort, that is 10-18 years old in 1999 (our cohort of interest). $\boldsymbol{\omega}_2$ captures rural area fixed effect. $\boldsymbol{\omega}_3$ is our co-efficient of interest where we have cohort dummy interacted with the rural dummy, capturing the deviation compared with urban areas. We also controlled for time-variant district effects by interacting the district dummy with the age dummy (capturing disproportionate change in education supply and demand in a particular year in some areas). V_j is the regional (district-level) time invariant fixed effects and $e_{i,j,t}$ is the error term. We estimate the equation using OLS (for years of education) and probit for completing different educational qualifications.

In Table 6 we reported the cohort analysis. We restricted our sample to those who were 10-18 years old in 1999 and the immediate older cohort of the same age-bracket (19-27 years old in 1999), making age-cutoff of 10-27 years old in 1999.[42] In column (1), in Panel A we reported regression estimates with years of education. Our estimates show that holding all other things constant, the 10-18 years old cohort of 1999 in rural area has 0.46 higher years of schooling, on average, compared to the same cohort located in urban areas. This impact is sizable and statistically significant. In Panel B, we disaggregated the age bracket into 10-12, 13-15 and 16-18 years old in 1999. Our estimates are consistent as found in Panel A and the impact is greater for the secondary school age (10-12 years old, 13-15 years old in 1999) in rural areas. In Column (2)-(4) we estimated probit regression with different stages of academic qualification, namely, Primary, Secondary and Higher Secondary. Our estimates show that the probability of finishing primary, secondary and higher secondary education increased by about 15, 16 and 14 percentage points, respectively, for the 10-18 years old rural cohort of 1999 compared with the base. In Panel B, we similarly disaggregated the age-bracket and consistent with the previous finding, we observe that mostly the secondary school aged children benefited the most from this exogenous shift in the examination calendar. To

[42]We could not use the same age-bracket for immediate younger cohort (1-9 years old in 1999), as this cohort is still continuing education during the 2016-17 HIES survey (18-26 years old in 2016).

Table 6: **Cohort Analysis 1: (Aged 10-27 in 1999)**

Variables	**Years of Education**	**Primary**	**Secondary**	**Higher Secondary**
Estimation:	**OLS**	**Probit**		
	(1)	(2)	(3)	(4)
Panel A:				
Aged 10-18 in 1999	10.48***	1.457***	0.951***	0.323***
	(0.160)	(0.047)	(0.045)	(0.049)
(Aged 10-18 in 1999) X (Rural)	0.462***	0.148***	0.161***	0.145**
	(0.112)	(0.030)	(0.032)	(0.037)
Rural	-2.155***	-0.458***	-0.603***	-0.697***
	(0.174)	(0.044)	(0.043)	(0.048)
Panel B:				
Age 10-12 in 1999	10.39***	1.417***	0.929***	0.302***
	(0.165)	(0.0504)	(0.0440)	(0.0474)
Age 13-15 in 1999	8.527***	1.058***	0.322***	-0.420***
	(0.155)	(0.0452)	(0.0427)	(0.0467)
Age 16-18 in 1999	7.619***	0.575***	-0.101**	-0.398***
	(0.186)	(0.0564)	(0.0510)	(0.0552)
(Age 10-12 in 1999) X (Rural)	0.587***	0.200***	0.191***	0.175***
	(0.139)	(0.0396)	(0.0360)	(0.0449)
(Age 13-15 in 1999) X (Rural)	0.773***	0.230***	0.222***	0.163***
	(0.141)	(0.0388)	(0.0412)	(0.0427)
(Age 16-18 in 1999) X (Rural)	-0.0272	0.000660	0.0563	0.0875
	(0.143)	(0.0432)	(0.0472)	(0.0582)
Rural	-2.155***	-0.458***	-0.603***	-0.697***
	(0.174)	(0.0436)	(0.0435)	(0.0483)
Other Control	Yes	Yes	Yes	Yes
District Control	Yes	Yes	Yes	Yes
District ×Age Control	Yes	Yes	Yes	Yes
Observations	49165	49129	49128	48987

Source: Compiled from HIES 2016 data. Notes: 1. Standard errors clustered at *district* level . reported in the parenthesis. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for cohort of birth dummy, sex, district and cohort of birth dummy interactions, and religion.

check the robustness of our analysis, we used age specific interaction with rural dummies which are reported in Table 7. As we can see from the Table 7 that the impact is mostly limited to 10 to 15 years of age in 1999 who got the full impact exposure of the academic calendar shift. Older age students also benefited but partially and only limited to higher secondary completion, as expected.

To understand the economic return of this impact, we estimated the private return of

Table 7: **Cohort Analysis 2: (Aged 10-27 in 2016)**

Variables	**Years of Education**	**Primary**	**Secondary**	**Higher Secondary**
Estimation:	**OLS**	**Probit**		
	(1)	(2)	(3)	(4)
(Aged 10 in 1999) X (Rural)	1.486***	0.424***	0.407***	0.311***
	(0.540)	(0.121)	(0.111)	(0.120)
(Aged 11 in 1999) X (Rural)	0.869**	0.212*	0.230*	0.330***
	(0.412)	(0.119)	(0.117)	(0.123)
(Aged 12 in 1999) X (Rural)	0.907**	0.278***	0.282***	0.344***
	(0.366)	(0.101)	(0.106)	(0.128)
(Aged 13 in 1999) X (Rural)	0.930*	0.335**	0.351***	0.230
	(0.486)	(0.137)	(0.131)	(0.150)
(Aged 14 in 1999) X (Rural)	1.337***	0.360***	0.364***	0.371***
	(0.357)	(0.0979)	(0.106)	(0.116)
(Aged 15 in 1999) X (Rural)	0.893**	0.227**	0.232*	0.257*
	(0.386)	(0.110)	(0.133)	(0.132)
(Aged 16 in 1999) X (Rural)	0.257	0.115	0.147	0.218*
	(0.391)	(0.117)	(0.117)	(0.125)
(Aged 17 in 1999) X (Rural)	0.414	0.0873	0.184	0.271*
	(0.444)	(0.127)	(0.117)	(0.139)
(Aged 18 in 1999) X (Rural)	0.413	0.0559	0.182	0.241*
	(0.406)	(0.121)	(0.123)	(0.146)
Other Control	Yes	Yes	Yes	Yes
District Control	Yes	Yes	Yes	Yes
District $\times$Age Control	Yes	Yes	Yes	Yes
Observations	49165	49129	49128	48987

Source: Compiled from HIES 2016 data. Notes: 1. Regression estimated using OLS with standard . errors clustered at *district* level reported in the parenthesis. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for cohort of birth dummy, sex, type of location (rural or urban), district and cohort of birth dummy interactions and religion.

education by employing Mincer (1974) type regression following the work of Montenegro and Patrinos (2014). We utilized HIES 2016 data for this estimation. Based on this framework, we regressed the natural logarithm of annual wage earning on age, age squared, sex, location (rural or urban) and regional dummy (district level) with standard errors clustered at the district level. Our estimates show that the economic return of one additional year of education is approximately 6.6 percent.[43] Plugging this number with our estimates indicate that

[43] This is consistent with Montenegro and Patrinos (2014) estimates on Bangladesh, which reported internal rate of return of 5.9 and 7.1 for 2000 and 2005, respectively for each additional year of schooling.

shifting academic calendar in favor of agricultural households led to an increase of about 3.03 percent in wages. This estimated economic return is comparable with other education related interventions like the Conditional Cash Transfer Program (CCT), pioneered in Mexico.

7 External Validity using India sample

As we mentioned in the introduction, the impact of the seasonal agricultural harvesting period overlapping with the school academic session (especially the grade completing exam) is an issue faced by many developing countries, especially agriculture-dominated ones. To check the cogency of this claim, one can conduct an external validity exercise — checking the impact of such an overlap in other countries. One promising candidate country to do such an analysis is India, a neighboring country to Bangladesh — where agriculture is a dominant sector for economic activities. Like Bangladesh, India also faces large school dropout rates.

In India, the state-supported public school system is the primary provider of school education. These state-supported schools are governed by the state-level academic calendars, where some states follow January to December academic sessions like Bangladesh. However, many states follow different academic calendars — depending on the locality, history, and climatic conditions (for example monsoon). Consequently, many states' academic calendars, especially the annual exam's timing, overlap with the primary crop harvesting period. Take Madhya Pradesh as an example, where wheat is the dominant crop of the state. The final examination timing of the public schools in Madhya Pradesh is in March, which overlaps with the wheat-harvesting season between February and April. However, many states, such as Bihar, observe no such overlap since the dominant crop of the state is rice, which is harvested between September and November, while the final exams are scheduled in March. Hence we can utilize such state-wise academic calendar variations to detect the impact of such overlap on school continuation for children from agricultural households.

To do this analysis we first generated Table 16 in the Appendix, where we reported state-specific dominant crops and their harvesting seasons for India, coupled with school academic sessions, final exam timing, and whether there is an overlap with the harvesting and academic calendar.[44]. In Table 16, state-wise agricultural information has been taken from GOI (2017) while academic session information has been taken from the GOI (2014). Second, we employed the panel-version of the Indian Human Development Survey (IHDS) data, collected in 2004-05 and 2011-12.[45] We begin with those interviewed in both the rounds of IHDS (N=150,988) to form the balanced panel. Unlike most education surveys, the IHDS collects information on a range of variables such as details on children's education, land-holding, employment, and economic status. To aid our analysis, we merged other variables such as rainfall, the area under crops, and major cereal production with the IHDS data. We obtained state-wise rainfall and cereal production information from the Indian Meteorological Department (IMD) and Directorate of Economics and Statistics, Government of India (GoI), respectively.

For our analysis, we took only those children who were enrolled in school during the first round of the IHDS survey and within the age range of 6 to 14 (and also 5 to 13 for robustness checks).[46] To identify agricultural households, we generated the agriculture dummy, which takes the value of one if the household head is employed in agriculture sector during the baseline, as classified in the IHDS survey. We defined "overlap-state" dummy where the harvesting time of the major crop overlaps with the annual school final exam based on the Table 16 in the Appendix.[47] We only considered the harvesting period of the dominant crop produced in the state, defined by the maximum share of gross cropped area allotted to that

[44]We could not use Chandigarh and Sikkim in our analysis due to data limitation.

[45]IHDS-1 interviewed 41,554 households (215,774 individuals) in 1503 villages and 971 urban neighborhoods across India. The second phase of the survey re-interviewed most of the households (N=42152) in 2011-12. To link the dataset from both rounds, we have followed instructions given on the IHDS website.

[46]Unlike Bangladesh analysis we could not use age 10-18 as our age-cutoff given the difference between the two survey waves is 7 years. According to the eighty-sixth amendment act (2002) the constitution of India provides free and compulsory education to all children in the age group of six to fourteen years, as a fundamental right.

[47]One caveat is Kerala where the major crop is rubber, which does not have a harvesting season in the traditional sense, hence we have taken rice (the second largest crop) as the representative crop for the state.

crop. Information on the academic session of different states is gathered from the Ministry of Human Resource Development, Government of India (GoI). Table 17 has the descriptive statistics of the sample used for the India analysis.

We estimated the impact of the state-specific overlapping calendar on school continuation using triple-difference regressions, where one difference is taken between the agricultural and non-agricultural households, one between overlapping and non-overlapping states and the other between the two survey rounds. Specifically, we use the following triple-difference specifications:

$$\begin{aligned}
\text{Enroll}_{iht} &= \alpha_1 \text{Year2011}_{ih} + \alpha_2 \text{Agri}_{ih} \times \text{Year2011}_{iht} \\
&+ \alpha_3 \text{Overlapping-State}_{ih} \times \text{Year2011}_{iht} \\
&+ \alpha_4 \text{Overlapping-State}_{ih} \times \text{Agri}_{ih} \times \text{Year2011}_{iht} \\
&+ \alpha_5 \text{X}_{iht} + \omega_h + \varepsilon_{iht}, \qquad \text{(G10)}
\end{aligned}$$

and

$$\begin{aligned}
\text{YrEdu}_{ih} &= \alpha_1 \text{Year2011}_{ih} + \alpha_2 \text{Agri}_{ih} \times \text{Year2011}_{iht} \\
&+ \alpha_3 \text{Overlapping-State}_{ih} \times \text{Year2011}_{iht} \\
&+ \alpha_4 \text{Overlapping-State}_{ih} \times \text{Agri}_{ih} \times \text{Year2011}_{iht} \\
&+ \alpha_5 \text{X}_{iht} + \omega_h + \varepsilon_{iht}, \qquad \text{(G11)}
\end{aligned}$$

where ω_h is the individual-specific fixed effects, ε is the idiosyncratic error term. Our main coefficient of interest is α_4 which is estimate of the triple difference variable Overlapping-State$\times$ Agri $\times$ Year2011$_{iht}$ in both equations.

Table 8 represents the regression estimates based on Equation G10. Column (1) reports the estimates of the basic triple difference estimator without any control. Columns (2) and (3) report the estimates with additional controls, where column (3) restricts the

Table 8: **External Validity: Enrollment estimates in India**

Dependent Variable: Change in Enrollment	**Age grouping: 5-13 in 2004**			**Age grouping: 6-14 in 2004**		
	Any members		**Nuclear Member**	**Any members**		**Nuclear Member**
	(1)	(2)	(3)	(4)	(5)	(6)
Year 2011	-0.259***	-0.154***	-0.131***	-0.288***	-0.168***	-0.139***
	(0.00703)	(0.0345)	(0.0322)	(0.00737)	(0.0342)	(0.0315)
(Agri) X (Yr 2011)	-0.0642***	-0.0469***	-0.0456***	-0.0675***	-0.0556***	-0.0558***
	(0.0116)	(0.0119)	(0.0133)	(0.0119)	(0.0122)	(0.0136)
(Overlapping state) X (Yr 2011)	-0.00678	-0.0348**	-0.0236	-0.00526	-0.0385***	-0.0283*
	(0.0117)	(0.0136)	(0.0152)	(0.0121)	(0.0142)	(0.0157)
(Overlapping state) X (Agri) X (Yr 2011)	-0.0534***	-0.0589***	-0.0655***	-0.0488**	-0.0499**	-0.0543**
	(0.0203)	(0.0198)	(0.0220)	(0.0208)	(0.0203)	(0.0225)
Other Household level Control	No	Yes	Yes	No	Yes	Yes
Observations	24378	22838	18922	24584	23014	19184
R-Square (within)	0.30	0.415	0.428	0.328	0.441	0.452
Mean of control group in 2011	0.74	0.74	0.72	0.72	0.72	0.69

Source: Compiled from IHDS 2004-05 and 2011-12 data. Notes: 1. Regression estimated using a panel fixed effect estimator with standard errors clustered at the household level.. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for time variant covariates: Age, age squared, parents age and education and number of household assets. We also control for State level major crop yields, area under agriculture as well as rainfall. 3. Time invariant variables are interacted with year 2011: agricultural household is an indicator variable for a household whose primary occupation is agriculture. Overlapping State) is an indicator variable of those states where major harvesting crop of the state overlaps with School annual exam period.

Table 9: **External Validity: Years of education estimates in India**

Dependent Variable: Change in Years of Education	**Age grouping: 5-13 in 2004**			**Age grouping: 6-14 in 2004**		
	Any members		**Nuclear Member**	**Any members**		**Nuclear Member**
	(1)	(2)	(3)	(4)	(5)	(6)
Year 2011	5.309***	3.864***	4.018***	5.220***	3.697***	3.878***
	(0.0379)	(0.141)	(0.156)	(0.0387)	(0.145)	(0.157)
(Agri) X (Yr 2011)	-0.0129	-0.0991*	-0.106	-0.0564	-0.138**	-0.144**
	(0.0588)	(0.0597)	(0.0672)	(0.0601)	(0.0600)	(0.0669)
(Overlapping state) X (Yr 2011)	-0.247***	0.131*	0.113	-0.201***	0.0696	0.0678
	(0.0616)	(0.0752)	(0.0830)	(0.0618)	(0.0751)	(0.0825)
(Overlapping state) X (Agri) X (Yr 2011)	-0.228**	-0.206**	-0.214*	-0.230**	-0.206**	-0.221*
	(0.101)	(0.101)	(0.113)	(0.102)	(0.102)	(0.113)
Other Household level Control	No	Yes	Yes	No	Yes	Yes
Observations	24378	22838	18922	24584	23014	19184
R-Square (within)	0.848	0.866	0.86	0.834	0.861	0.856
Mean of control group in 2011	8.00	8.00	8.00	8.24	8.24	8.24

Source: Compiled from IHDS 2004-05 and 2011-12 data. Notes: 1. Regression estimated using a panel fixed effect estimator with standard errors clustered at the household level. $*$, $**$, $***$ indicate significance levels at 10%, 5%, 1%, respectively. 2. Regression estimates control for time variant covariates: Age, age squared, parents age and education and number of household assets. We also control for State level major crop yields, area under agriculture as well as rainfall. 3. Time invariant variables are interacted with year 2011: agricultural household is an indicator variable for a household whose primary occupation is agriculture. Overlapping State) is an indicator variable of those states where major harvesting crop of the state overlaps with School annual exam period.

sample among the nuclear members of the household head. All the standard errors reported in the Table 8 are clustered at the household level. As we can see, the year 2011 dummy and agriculture household interacted with the year 2011 dummy show negative impact on enrollment, demonstrating the natural dropout trend and vulnerability of the poor agricultural household students. After controlling for these effects, we see a sizable negative impact on enrollment in 2011 for those agricultural household children who were in schools in the overlapping states. This impact is highly statistically significant and sizable, causing about 5.36 percentage point decline in the enrollment from the mean. We used a different age bandwidth (age 6-13 years old in 2004) in columns (4)-(6) of Table 8 which show similar estimates. Given the population of India, this estimate is sizable, causing millions of children to discontinue schooling due to calendar mismatch. In Table 9 we estimated equation G10 with years of education as a dependent variable and found similar negative impacts, about 20.5 to 26.3 percentage points less years of schooling by agricultural household children in overlapping states — demonstrating external validity of our Bangladesh finding.

8 Conclusion

Seasonality in agrarian societies is an important issue, and must be appropriately addressed to formulate effective public policies. Surprisingly, seasonally adjusted policies outside the context of food security and disaster management are rare. This paper addresses the impact of seasonal labor demand on school continuation in Bangladesh. The school calendar for both primary and secondary schools in Bangladesh is not seasonally adjusted to local agricultural cycles. In particular, the annual final examination of schools typically overlaps with the *Aman* rice harvest in December, when local agricultural labor demand is on the rise.

By using household-level panel data and a natural experiment framework where *Ramadan* driven holidays in 1999 forced schools to re-schedule final examinations to the pre-harvest season, and comparing it with a typical year of 2002, this paper assessed the impacts of such overlap between school exams and the harvest period. Our estimated results robustly indicate that children from agricultural households benefitted significantly in their school continuation from the *Ramadan*, due to a favorable off-harvest exam schedule in 1999 compared with children from non-agricultural households. In other words, a favorable annual examination schedule, away from the harvest season, helped school going children from agricultural households to continue schooling in 1999. However, due to the typical unfavorable examination schedule that overlaps with harvesting, which was observed in 2002, we see a larger drop in enrollment. Exploiting state level academic calendar variation, we executed a similar analysis of school enrolled children in India and found supporting evidence, demonstrating external validity of our claim.

Our results indicate that rescheduling the annual examinations away from the peak labor demand period to address local seasonality may increase school continuation for school students from agricultural households in Bangladesh. Using nationally representative household survey we estimated that this shifting in exam calendar for the 10-18 years old rural cohort has increased years of education by 0.46 years. Our estimates also indicate this led to increasing the probability of completing primary, secondary, and higher secondary school by 15, 16 and 14 percentage points, respectively, compared to the same urban cohort. Using the 2016 HIES, we estimated the return to education in Bangladesh and based on our estimates, this 0.46 additional years of education has a sizable economic return—about 3 percent increase in income. To benchmark this effect, the pioneering CCT program of Mexico, Progresa-Oportunidades yielded 0.66 additional years of schooling for every 8 years of participation in the program (UNESCO 2006). Compared with this our favorable calendar shift continued for three years. Infrastructural interventions such as large-scale school construction program in Indonesia yielded an increase of 0.12-0.19 years of education and 3 to 5.4 percent economic return (Duflo 2001). Compared to these interventions, fixing academic calendar to avoid seasonal labor demand is a cost-effective intervention with sizable return.

Beyond seasonality, ample factors hamper education performance in developing countries.

However, adjusting the school calendar to accommodate local agrarian calendars can reduce the dilemma faced by children from agricultural households. Moreover, such an adjustment involves a relatively small, one-off cost of a curriculum change. The United Kingdom implemented a seasonally adjusted school calendar during World War II, and the impacts were regarded as favorable, although the presented results were anecdotal (Moore-Colyer, 2004, 190-191). In early 20th-century Japan, the school calendar was adjusted to accommodate daytime work hours, and some students were allowed to attend night school or take shorter courses (Institute for International Cooperation, 2004, Chapter 3). Even in Bangladesh, non-formal education providers, primarily non-governmental organizations (NGOs), have taken steps to adjust school calendars according to seasonality. For instance, schools run by BRAC, a leading NGO, have begun using a seasonally adjusted school calendar for non-formal education in Bangladesh.

One could reasonably argue that providing a well targeted subsidy akin to conditional cash transfer (CCT) is one way to achieve the goal of retaining children from agricultural households in schools. Policy makers may also consider alternative measures like targeted CCT during the peak labor demand season—reducing the pull factor for the children from poor agricultural households. However, we argue that school calendar flexibility with local economic activities is more advisable as a policy suggestion for two important reasons. First, one should pay attention to ensure that children's time use is never dichotomous of schooling or working; on the contrary, a substantial number of children are required to do both, at least in the period of rising seasonal labor demand such as during harvesting. This reflects the reality that it may be prohibitively costly to eliminate profitable activities. Second, fixing the school calendar to accommodate seasonality will be a less expensive and administratively easier solution than providing a well targeted subsidy.[48] Given these considerations, we expect the results of our empirical analysis to have reasonable credibility, thereby providing

[48]Intuitively the subsidy amount that we would need to provide in this case would be equal to (incentive amount) * (days of harvest) * (number of rural children enrolled) and administrative costs for implementation at school and local government, both of which would be financed with deficits.

the foundation for a school calendar reform that benefits children in agrarian economies like Bangladesh and other countries, globally.

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9 Appendix A1: Descriptive statistics

Data we used in our paper is drawn from IFPRI panel surveys of 1999, 2002, and 2006. Data from 1999 and 2002 are used in the main regression estimations where the 2006 (as well as other rounds) data set is used for placebo and parallel trend tests. Given our focus on those children actively engaged in agricultural works, we set the lowest age limit as 10 years of age - based on the definition of child labor used in the Labor Force Survey (LFS) of Bangladesh. [49]. Setting the upper age limit for our sample is not as simple as setting the lower age limit. Children are officially supposed to finish high school at the age of 16 years, but as a result of starting late and repeating grades, many individuals remain in high school beyond that age. As public primary schools accept children up to the age of 10 years for grade 1 and because many children begin enrolling late, several individuals who may be considered "adults", if judged according by their age alone, are still attending secondary or high schools.

Under these conditions, the oldest individual in our sample was 18 years old in 1999. The lower and upper age limit of 10-18 years was applied, which makes the sample 838 individuals.[50] We exclude individuals whose highest education level in round 2 (2002) is preschool, madrasa (Islamic religious schools), or bachelor's or higher degrees. Thus, our sample essentially becomes 735.

For our regression exercise, we utilize only the balanced covariate portion of the 1999-2002 panel, which cuts the sample from 735 to 682 individuals. Table 10 shows that the trimmed sample is not systematically different from the original sample (except for child's age), hence negligible concern on the sample selection issue, due to the use of balanced covariate portion of the 1999-2002 panel. In our main regression specification we controlled for child's age. In addition, our main findings of this study remain largely consistent if we use the 735 sample and these estimates are available upon request.

[49]In Bangladesh, the official age to begin schooling is six years of age. However, some parents choose to begin later. As a result many of our sample our still in the primary grades despite their age is suitable for the post-primary grades.

[50]There are 2,597 individuals in the original panel data.

Table 11 presents a summary of the data used in the regressions. Based on the age cutoff of 10 years and older in the 1999 survey, there are 682 observations. Mean enrollment rate is 73 percent. In our sample we have approximately 60 percent agricultural households. Alternative agricultural household definitions give similar summary statistics, which explains the small difference in estimation. The household head's highest level of education is mostly primary, 36%; those with a secondary level of education comprise 23.0% of our sample. Spousal education is similar for the primary level, 32%, but lower for the secondary level, 26.0%. Mean per member landholding is 0.175 acre. Median non-land assets per member are about 7,000 BDT (83 USD), and 14,000 BDT (167 USD) at the 75th percentile. These non-land asset values indicate that our sample primarily consists of poor rural households.

For placebo sample we used 10 to 18 years old in 2002. Table 12 below presents a summary of the data used in the placebo regressions. Based on the age cutoff of 10 years and older in the 2002 survey, there are 870 observations in the placebo sample. In our placebo sample we have about 58% agricultural households, which is quite similar with the main sample. Other summary statistics reported in Table 12 show strong similarity with Table 11 demonstrating the validity of the placebo sample. The only observable difference between these two samples is the declining enrolment rate, which is about 11% lower than the 1999 average.

We first did a tabulation of agriculture and non-agricultural households based on household consumption information in Table 13. The consumption quartiles are derived based on per-member consumption information in the household. We noticed higher consumption quartile for non-agricultural households compared to Agricultural households.

The IFPRI panel data-set reports reasons for dropping out of school, which are presented in TABLE 14. We summarize the reported reasons for dropping out by consumption quartiles as reported in Table 13. As we can notice, dropout rates are higher for lower consumption quartiles, and their reasons for dropping out primarily include financial difficulties, which is also true for irregular students. Upper-quartile individuals cite non-financial reasons such as marriage as the reason for drop-out.

In Table 15, we summarize the reported reasons by household type: agricultural or non-agricultural households. Our table indicates that agricultural households cite financial reasons more frequently than non-agricultural ones as the main reason for dropping out and school irregularity. These findings suggest that we must control for household wealth in analyzing school enrollment decisions.

10 Appendix A2: Tables

Figure 4: Government School Calendar in Bangladesh (2019).[a]

Junior and Secondary School Holiday List:

বিষয় : সরকারি/বেসরকারি মাধ্যমিক ও নিম্নমাধ্যমিক বিদ্যালয়সমূহের ২০১৯ শিক্ষাবর্ষের ছুটির তালিকা ও শিক্ষাপঞ্জী অনুমোদন।

সূত্র : মাধ্যমিক ও উচ্চ শিক্ষা অধিদপ্তরের স্মারক নং-ওএম/১৮৮-সম/২০০২/২৯৪৬; তারিখ: ১৮.১১.২০১৮

উপর্যুক্ত বিষয় ও সূত্রোক্ত পত্রের পরিপ্রেক্ষিতে জানানো যাচ্ছে যে, সরকারি/বেসরকারি মাধ্যমিক ও নিম্নমাধ্যমিক বিদ্যালয়সমূহের ২০১৯ শিক্ষাবর্ষের ছুটির তালিকা ও শিক্ষাপঞ্জী সরকার নিম্নোক্তভাবে অনুমোদন করেছে:

ক্রমিক	পর্বের নাম	তারিখ ও দিন	তারিখ বঙ্গাব্দ	দিন সংখ্যা
১.	শ্রী শ্রী সরস্বতী পূজা	১০ ফেব্রুয়ারি, রবিবার, ২০১৯	২৭ মাঘ, ১৪২৫	০১ দিন
২.	* মাঘী পূর্ণিমা	১৯ ফেব্রুয়ারি, মঙ্গলবার, ২০১৯	০৬ ফাল্গুন, ১৪২৫	০১ দিন
৩.	শহীদ দিবস ও আন্তর্জাতিক মাতৃভাষা দিবস	২১ ফেব্রুয়ারি, বৃহস্পতিবার, ২০১৯	০৮ ফাল্গুন, ১৪২৫	০১ দিন
৪.	শ্রী শ্রী শিবরাত্রি ব্রত	০৪ মার্চ, সোমবার, ২০১৯	১৯ ফাল্গুন, ১৪২৫	০১ দিন
৫.	জাতির পিতা বঙ্গবন্ধু শেখ মুজিবুর রহমান এর জন্ম দিবস	১৭ মার্চ, রবিবার, ২০১৯	০৩ চৈত্র, ১৪২৫	০১ দিন
৬.	শুভ দোলযাত্রা	২১ মার্চ, বৃহস্পতিবার, ২০১৯	০৭ চৈত্র, ১৪২৫	০১ দিন
৭.	স্বাধীনতা ও জাতীয় দিবস	২৬ মার্চ, মঙ্গলবার, ২০১৯	১২ চৈত্র, ১৪২৫	০১ দিন
৮.	* শব-ই-মিরাজ	০৪ এপ্রিল, বৃহস্পতিবার, ২০১৯	২১ চৈত্র, ১৪২৫	০১ দিন
৯.	বৈসাবি	১২ এপ্রিল, শুক্রবার, ২০১৯	২৯ চৈত্র, ১৪২৫	০০ দিন
১০.	বাংলা নববর্ষ	১৪ এপ্রিল, রবিবার, ২০১৯	০১ বৈশাখ, ১৪২৬	০১ দিন
১১.	* শব-ই-বরাত, ইস্টার সানডে	২১ এপ্রিল, রবিবার, ২০১৯	০৮ বৈশাখ, ১৪২৬	০১ দিন
১২.	মে দিবস	০১ মে, বুধবার, ২০১৯	১৮ বৈশাখ, ১৪২৬	০১ দিন
১৩.	গ্রীষ্মকালীন অবকাশ, * পবিত্র রমজান, * বুদ্ধ পূর্ণিমা (বৈশাখি পূর্ণিমা ১৮মে) জুমাতুল বিদা (৩১ মে), * শব-ই-কদর (০২ জুন), * ঈদ-উল-ফিতর (০৫ জুন)	০৬ মে, সোমবার থেকে ১৩ জুন বৃহস্পতিবার, ২০১৯	২৩ বৈশাখ থেকে ৩০ জ্যৈষ্ঠ ১৪২৬	৩৪ দিন
১৪.	* পবিত্র ঈদ-উল-আযহা (১১, ১২, ১৩ আগস্ট), জাতীয় শোক দিবস (১৫ আগস্ট)	০৮ আগস্ট, বৃহস্পতিবার থেকে ১৯ আগস্ট, সোমবার, ২০১৯	২৪ শ্রাবণ থেকে ০৪ ভাদ্র, ১৪২৬	১০ দিন
১৫.	শুভ জন্মাষ্টমী	২৩ আগস্ট, শুক্রবার, ২০১৯	০৮ ভাদ্র, ১৪২৬	০০ দিন
১৬.	* হিজরী নববর্ষ	০১ সেপ্টেম্বর, রবিবার, ২০১৯	১৭ ভাদ্র, ১৪২৬	০১ দিন
১৭.	* আশুরা	১০ সেপ্টেম্বর, মঙ্গলবার, ২০১৯	২৬ ভাদ্র, ১৪২৬	০১ দিন
১৮.	দুর্গাপূজা (বিজয়া দশমী, ০৮ অক্টোবর) * প্রবারণা পূর্ণিমা (১৩ অক্টোবর), শ্রী শ্রী লক্ষ্মী পূজা(১৩ অক্টোবর)	০৪ অক্টোবর, শুক্রবার থেকে ১৩ অক্টোবর, রবিবার, ২০১৯	১৯ আশ্বিন থেকে ২৮ আশ্বিন, ১৪২৬	০৮ দিন
১৯.	* আখেরী চাহার সোম্বা	২৩ অক্টোবর, বুধবার, ২০১৯	০৭ কার্তিক, ১৪২৬	০১ দিন
২০.	শ্রী শ্রী শ্যামা পূজা	২৭ অক্টোবর, রবিবার,২০১৯	১১ কার্তিক, ১৪২৬	০১ দিন
২১.	* ঈদ-ই- মিলাদুন্নবী (সাঃ)	১০ নভেম্বর, রবিবার, ২০১৯	২৫ কার্তিক, ১৪২৬	০১ দিন
২২.	*ফাতেহা-ই-ইয়াজদাহম	০৯ ডিসেম্বর, সোমবার ২০১৯	২৪ অগ্রহায়ণ, ১৪২৬	০১ দিন
২৩.	শীতকালীন অবকাশ, বিজয় দিবস(১৬ ডিসেম্বর), যিশু খ্রিস্টের জন্মদিন (বড় দিন, ২৫ ডিসেম্বর)	১৫ ডিসেম্বর, রবিবার থেকে ২৯ ডিসেম্বর, রবিবার, ২০১৯	৩০ অগ্রহায়ণ থেকে ১৪ পৌষ, ১৪২৬	১৩ দিন
২৪.	প্রধান শিক্ষকের সংরক্ষিত ছুটি			০৩ দিন
		মোট =		৮৫ দিন

* চাঁদ দেখার উপর নির্ভরশীল।

<u>পরীক্ষার সময়সূচি-২০১৯ খ্রি:</u>

পরীক্ষার নাম	তারিখ	দিন সংখ্যা	ফলাফল প্রকাশ
অর্ধ-বার্ষিক/প্রাক নির্বাচনী পরীক্ষা	২২ জুন, শনিবার থেকে ০৪ জুলাই, বৃহস্পতিবার পর্যন্ত ২০১৯	১২ দিন	২০ জুলাই, শনিবার, ২০১৯
নির্বাচনী পরীক্ষা	১৪ অক্টোবর, সোমবার থেকে ২৯ অক্টোবর, মঙ্গলবার পর্যন্ত ২০১৯	১২ দিন	০৭ নভেম্বর, বৃহস্পতিবার ২০১৯
বার্ষিক পরীক্ষা	২৭ নভেম্বর, বুধবার থেকে ১১ ডিসেম্বর, বুধবার পর্যন্ত ২০১৯	১২ দিন	৩০ ডিসেম্বর, সোমবার ২০১৯

[a] Above is the Education Ministry of Bangladesh provided examination and annual holiday calendar for the secondary and higher secondary schools (in Bangla). The second table of this notice contains the exam calendar. The notice instructed all the schools to hold annual exams from November 27th to 11th December, 2019.

Table 10: Original vs. regression data contrasts: main sample

Variables	Original Sample	Regression Sample	T-stat	χ^2	Binomial
Agricultural household	0.6027	0.5968	0.8196	0.8618	0.7544
Agricultural household (head)	0.5619	0.5587	0.9020	0.9444	0.8774
Child's Age	13.4313	13.0132	0.0025	NA	NA
Child's Sex (female = 1)	0.5007	0.5103	0.7187	0.7586	0.6189
Head education: primary	0.3537	0.3592	0.8293	0.8726	0.7793
Head secondary: secondary	0.2381	0.2346	0.8773	0.9267	0.8574
Spouse's sex (female = 1)	0.9293	0.9296	0.9785	1.0000	1.0000
Spouse education: primary	0.3156	0.3167	0.9655	1.0000	0.9671
Spouse education: secondary	0.2544	0.2610	0.7776	0.8243	0.6924
Per member land holding (Acre, in 1999)	0.1814	0.1751	0.6886	NA	NA
Per member non-land asset (1000 BDT, in 1999)	11.5303	11.4060	0.8710	NA	NA
Paddy yield (annual, thana level)	0.7834	0.7841	0.9066	NA	NA
Household has structured toilet (dummy)	0.2925	0.2933	0.9757	1.0000	0.9664
Household has access to piped water (dummy)	0.3687	0.3724	0.8847	0.9282	0.8428
Observations	735	682			

Notes: All information is based on first round of data set, which was collected in 1999. Column headed by $T - Stat$ shows p values of equal means for both data sets using t tests. Column headed by χ^2 shows p values of equal proportions. Column headed by Binomial shows p values of two-sided test for one proportion being equal to another proportion under presumed Bernoulli trials. Agricultural household are defined as at least one adult member claiming that main income source as agriculture, agricultural household (head) is defined as head is claiming that main income source is agriculture. Age and sex are of children of the households.

Table 11: Descriptive Statistics: Main sample

Variables	Min	25%	Median	75%	Max	Mean	STD	'0's	'NA's	n
Enrolled	0	0	1	1	1	0.73	0.44	184	0	682
Agricultural household	0	0	1	1	1	0.6	0.49	275	0	682
Agricultural household (head)	0	0	1	1	1	0.56	0.5	301	0	682
Program membership	0	0	1	1	1	0.73	0.44	183	0	682
Child's Age	10	11	13	15	18	13.01	2.38	0	0	682
Child's Sex (female = 1)	0	0	1	1	1	0.51	0.5	334	0	682
Head education: primary	0	0	0	1	1	0.36	0.48	437	0	682
Head secondary: secondary	0	0	0	0	1	0.23	0.42	522	0	682
Spouse education: primary	0	0	0	1	1	0.32	0.47	466	0	682
Spouse education: secondary	0	0	0	1	1	0.26	0.44	504	0	682
Per member land holding (acre, in 1999)	0	0.018	0.066	0.19	3.215	0.168	0.294	2	0	682
Per member non-land asset (1000 BDT, in 1999)	0.369	3.537	6.963	13.557	205	11.017	14.649	0	0	682
Paddy yield (annual, thana level)	0.61	0.65	0.82	0.91	0.93	0.78	0.11	0	0	682
Household has Structured toilet (dummy)	0	0	0	1	1	0.29	0.46	482	0	682
Household has access to piped water (dummy)	0	0	0	1	1	0.37	0.48	428	0	682
Mean low temperature (annual, thana level)	20.77	21.24	21.32	22.35	22.59	21.6	0.63	0	0	682
Mean high temperature (annual, thana level)	30.11	30.69	31.18	31.61	32.33	31.15	0.66	0	0	682

Notes: All information is based on first round of data set, which was collected in 1999. Agricultural household is an indicator variable for a household whose primary income is agriculture. Agricultural household (head) is defined as head is claiming that main income source is agriculture. Program membership is 1 if at least one household membership receive either safety net or education related program (stipend/scholarship) support. Age and sex are of children of the households. STD represents Standard deviation.

Table 12: Descriptive Statistics: placebo sample

Variables	Min	25%	Median	75%	Max	Mean	STD	'0's	'NA's	n
Enrolled	0	0	1	1	1	0.62	0.48	327	0	870
Agricultural household	0	0	1	1	1	0.58	0.49	362	0	870
Agricultural household (head)	0	0	1	1	1	0.55	0.5	388	0	870
Program membership	0	0	0	1	1	0.26	0.44	640	0	870
Child's Age	10	12	13	16	18	13.64	2.47	0	0	870
Child's Sex (female = 1)	0	0	1	1	1	0.53	0.5	413	0	870
Head education: primary	0	0	0	1	1	0.34	0.48	570	0	870
Head secondary: secondary	0	0	0	0.75	1	0.25	0.43	652	0	870
Spouse education: primary	0	0	0	1	1	0.27	0.45	632	0	870
Spouse education: secondary	0	0	0	1	1	0.27	0.45	632	0	870
Paddy yield (annual, thana level)	0.69	0.74	0.84	0.98	1.04	0.85	0.12	0	0	870
Household has Structured toilet (dummy)	0	0	0	1	1	0.28	0.45	624	0	870
Household has access to piped water (dummy)	0	0	0	1	1	0.37	0.48	546	0	870
Mean low temperature (annual, thana level)	20.5	20.77	20.98	21.79	22.52	21.23	0.67	0	0	870
Mean high temperature (annual, thana level)	29.31	29.47	30.41	30.75	31.95	30.38	0.81	0	0	870

Notes: All information is based on second round of data set, which was collected in 2002. Agricultural household are defined as at least one adult member claiming that main income source as agriculture, agricultural household (head) is defined as head is claiming that main income source is agriculture. Program membership is 1 if at least one household membership receive either safety net or education related program (stipend/scholarship) support. Age and sex are of children of the households. STD represents Standard deviation.

Table 13: Tabulation of Agricultural vs. Non-Agriculture household Consumption Quartiles

Quartiles	1	2	3	4	'NA's	N
Agricultural households	25.8	27.27	26.54	19.66	0.74	407
Non-agricultural households	23.27	24.36	18.55	33.82	0	275

Notes: Consumption quartiles are based on households.

Table 14: Reported Reasons for Stop Going to School by Consumption Quartiles and by Household Type

Quartile	Group	Financial	Not accepted	School environment	Marriage	Distance	Sickness	NA	Total
1	Irregular 1999	0.52	0.03	0.2	0	0.06	0.02	0.17	65
1	Irregular 2002	0.54	0.01	0.01	0.01	0.01	0	0.43	115
1	Drop outs 2002	0.62	0	0.02	0.02	0	0	0.34	58
2	Irregular 1999	0.48	0	0.23	0	0.14	0	0.14	56
2	Irregular 2002	0.37	0	0.06	0.01	0.02	0.01	0.52	81
2	Drop outs 2002	0.41	0	0.08	0	0.03	0	0.49	37
3	Irregular 1999	0.44	0	0.26	0	0.18	0.09	0.03	34
3	Irregular 2002	0.28	0	0	0.05	0.03	0.03	0.61	64
3	Drop outs 2002	0.26	0	0	0.05	0.03	0.03	0.63	38
4	Irregular 1999	0.45	0.05	0.18	0	0.05	0.05	0.23	22
4	Irregular 2002	0.1	0.01	0.03	0.03	0.01	0.03	0.79	73
4	Drop outs 2002	0.11	0.02	0.02	0.04	0	0.04	0.78	54

Notes: Numbers are all ratios except totals. "Agri HH" indicates agricultural households. See main text for definition of agricultural households. Non-goers are individuals who were not enrolled in respective period. Drop outs are individuals who were enrolled in 1999 but not in 2002..

Table 15: Reasons for Not Going to School, Agricultural vs. Non-Agriculture household

HH Type	Group	Financial	Not accepted	School Environment	Marriage	Distance	Sickness	NA	Total
Ag	Irregular 1999	0.55	0.03	0.21	0	0.06	0.05	0.11	66
Ag	Irregular 2002	0.38	0	0.01	0.01	0.01	0.02	0.57	120
Ag	drop outs 2002	0.46	0	0.01	0	0.01	0.01	0.5	70
Non-ag	Irregular 1999	0.46	0.01	0.22	0	0.13	0.02	0.16	112
Non-ag	Irregular 2002	0.33	0.01	0.03	0.03	0.02	0.01	0.56	214
Non-ag	drop outs 2002	0.3	0.01	0.03	0.04	0.01	0.02	0.59	117

Notes: Numbers are all ratios except totals. "Agri HH" indicates agricultural households. See main text for definition of agricultural households.
Non-goers are individuals who were not enrolled in respective period. Drop outs are individuals who were enrolled in 1999 but not in 2002..

Table 16: India State Wide Academic and Agricultural Calender

States	Major Crop	Area under crop (% of cropped area)	Academic Session	Final Exam	Harvest Period of major crop	Final Exam timing overlaps with Harvesting period
Andhra Pradesh	Rice	30%	June to April	April/May	November December	No
Arunachal Pradesh	Rice	47%	July to April	February	November-December	No
Assam	Rice	62%	January to December	December	November December	Yes
Bihar	Rice	44%	April to March	March	September to November	No
Delhi	Wheat	45%	April to March	March	March-April	Yes
Goa	Rice	29%	June to April	March	September-October	No
Gujarat	Cotton	23%	June to May	March	October to April	Yes
Haryana	Wheat	39%	April to March	Feb-March	April-May	No
Himachal Pradesh	Wheat	38%	April to March	March	April-June	No
Jammu-Kashmir	Wheat	26%	November to October	October-November	April-June	No
Jharkhand	Rice	69%	April to June	January-March	September to November	No
Karnataka	Pulse	18%	May to April	March	November-January	No
Kerala	Rice	8%	June to March	March	September-October	No
Madhya Pradesh	Wheat	23%	July to April	March	February-April	Yes
Maharashtra	Cotton	19%	June to May	March	Nov-Jan	No
Manipur	Rice	61%	Feb to Jan	December-January	October-November	No
Meghalaya	Rice	32%	Feb to Jan	October	October-December	Yes
Mizoram	Rice	26%	Jan to Dec	November-December	October-December	Yes
Nagaland	Rice	38%	Jan to Dec	November-December	September-November	Yes
Odisha	Rice	83%	April to March	March	September-October	No
Puducherry	Rice	63%	June to April	March-April	September-October	No
Punjab	Wheat	45%	April to March	March	April-May	No
Rajasthan	Pulse	16%	July to June	March	Feb-March	Yes
Tamil Nadu	Rice	33%	June to April	March-April	September-October	No
Tripura	Rice	53%	Jan to Dec	November-December	September-November	Yes
Uttar Pradesh	Wheat	38%	July to June	February-March	March-April	Yes
Uttarakhand	Wheat	31%	April to March	March-April	March-April	Yes
West Bengal	Rice	58%	Feb to Dec	November-December	August-November	Yes

Table 17: Descriptive Statistics: India sample (2004)

Variables	Mean	STD	Max	Min
Enrolled	1.00	0.00	1	1
Completed Years of education	2.82	2.18	0	12
Agricultural Households	0.385	0.485	0	1
Overlapping State	0.352	0.477	0	1
Age	9.10	2.38	5	13
Mother's Age	33.95	6.32	18	80
Father's Age	38.79	7.03	21	88
Father's Education	2.15	3.43	0	15
Mother's Education	4.44	4.44	0	16
No. of Assets	8.78	4.55	0	25
Household Size	6.67	2.72	2	38

Notes: All information is based on first round of data set, which was collected in 2004. Agricultural household is an indicator variable for a household whose primary income is agriculture. Agricultural household (head) is defined as head is claiming that main income source is agriculture. STD represents Standard deviation.